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ART. I.—*On the Origin of Species;* by THEOPHILUS PARSONS, Dane Professor of Law in Harvard University, Cambridge, Massachusetts.

IT has frequently occurred in the history of science that some startling theory, which, when first announced, was regarded as the antagonist of received opinions, and became at once the subject of earnest hostility as well as unqualified approbation, has, after much discussion been importantly qualified and modified, and thus reconciled with views which it seemed to contradict; and when thus shorn of its excess and moderated in its demands, has been generally adopted as an important addition to knowledge. It may yet be so with Mr. Darwin's views.

His theory, stated very briefly, is, that all organisms tend to reproduce themselves in a geometrical ratio, and with such exuberance of life, that each one would speedily fill the earth, if not repressed by constant and powerful causes of destruction. Hence but a very small proportion of seeds or ova which are impregnated are able to mature and reproduce. Therefore there must be a competition, or as he phrases it, a "struggle for life," among all these impregnated germs of life; and if one in a hundred only lives there must be a reason why that one lives rather than the ninety and nine which perish. This reason must again be frequently, or at least sometimes, that it had some advantage in this "struggle for life," by a structural or functional difference. That is, it varied from its kindred, in such wise, that it was somewhat easier for it to live, to grow, to mature, and to reproduce, than for them. This difference or variation it must, as a general rule, impart to its offspring. When it be-

came established, the same law of struggle, of advantage, of life, and of death, would operate upon this new and improved variety, and would cause another and a farther improvement. As this law is universal, and must always have operated upon all organisms from the beginning, not only are varieties established in this way, but so likewise varieties become species, species become genera, and so also orders, classes, families are formed, and thus finally we may suppose that all the organisms of the earth, living and extinct, animal and vegetable, have proceeded from the simplest original form of life.

While much interested in Darwin's work and in the discussions and controversies to which it has given rise, it occurred to me to consider whether one of the limitations which he seems to have imposed upon himself, was necessary. He assumes, and reasons exclusively upon the assumption, that the successive changes by which these great results have been brought about have always been minute and slow, and have only become sufficient to reach their consummation, by an indefinite accumulation of effects, through the indefinite periods of time which geology affords them. It seemed to me that this assumption was quite unnecessary, and therefore unphilosophical; and supposing that these changes may sometimes have been much greater, I then inquired what would be the effect of this supposition upon the general theory, that the succession of organized being has from the beginning been produced by generative development. This paper is intended to suggest—and only to suggest—some of the results to which I have come. Upon the question whether I have not departed so widely from the theory of Darwin, that I have no right to use his name, I have nothing to say. I wish only that these suggestions may pass for what they are worth whatever that may be.

To say that it is the tendency of all organisms to reproduce their like, but with some difference, would be merely to utter a truism, for there is almost or quite always some family resemblance between offspring of the same parents, and always so much of difference that no two of the offspring are ever undistinguishable from each other. We may say, however, that one certain law of this difference, or variation, is this; that while a slight difference is universal, great difference is less common, and the greater the difference the more rare it is, and therefore the less to be expected in any given instance. The question then arises, how far this difference may go; or to say the same thing in other words, what limit is there to the possible immediate variation of offspring from their parents and kindred?

The law of variation is itself variable; and while we have little knowledge of the causes of variation, we have none whatever of the limits to which it may be carried. Indeed, if we assume that there must be some limit to the possible extent of

variation, we may infer that it must be a very broad one, from the instances of extreme monstrosity which science has recorded. Let us say, then, that we will assume that there may be as much variation or aberration as these records prove that there has been, and no more.

Perhaps abnormality always seems to us a mischief, and by monstrosity we always understand aberration in a wrong direction; and facts would justify the inference that extreme aberration is usually a degradation. But we have no sufficient reason for saying that this is a *law;* or, in other words for asserting, that there can never be monstrosity in a right direction; or in yet other words, that the aberrance can never be an improvement and a help. As this seems to me an important principle I restate it. We know that it is the tendency of all organisms to reproduce their kind, but with some difference. We know, for all the improvement in our domestic animals proves it, that this difference may be improvement. We know that this difference may be carried to an enormous extent, as a mischief, because the records of monstrosity prove it; and we do not know that this difference may not be carried to an equal extent in the opposite direction of improvement.

My position therefore is precisely this. It is always possible that offspring may be born, differing as much from their parents and kindred in the way of gain, of advantage, and of improvement, as we know that offspring have differed in the way of loss, of hindrance and of degradation; and therefore when I speak of extreme aberration I shall mean by it variation carried to this extent.

Admitting this principle as possible, let us proceed with it to consider what may be called the system of Agassiz; using his name only because he has given to it great development and full illustration.

Take first his assertion that there must have been in each geological age many new creatures; say if you please an hundred or a thousand, and consider this as proved and admitted. Still it leaves wholly untouched the question how these new creatures were created. And be the answer what it may, that answer so far as it is only an answer to this question, leaves the assertion of Agassiz untouched. But if we bring to the question, how were these creatures created? the possibility of aberrant variation of offspring in the direction of improvement, we bring to it one answer. For example: suppose the time to have come when there is to be a new creation, and it is to be a dog, or rather two dogs, which will be the parents of all dogs. How shall they be created? We may say of this either of five things. One is, that we do not know, and never can know, and had better not inquire. This does not seem any answer. A second is, that they will be created "by chance." This also seems to me no answer, because chance is a word only and not a thing. A

third is, that they will be created at once and out of nothing, by the absolute will of a creator. This answer does not satisfy me much better. The fourth is, that they will be so created by absolute fiat, out of a proper quantity of earth and water, with the necessary chemical elements in due proportion, which had been summoned to meet together in a proper place by the will of the Creator for that purpose. But this answer does not recommend itself to my reason much more than the others. The fifth is, he will be created by some influence of variation acting upon the ovum (before or at conception or during its uterine nutriment) of some animal nearest akin—a wolf, a fox, a hyena, or a jackal; and the brood will come forth puppies and grow up dogs to produce dogs. Now the question is not whether this last answer offers a probability *per se*, but whether it is not after all less improbable than either of the other suppositions; less unphilosophical than either of the other answers, and therefore to be accepted on that ground: and I may say in passing, that if the present favorite theory for accounting for the diversities of our domestic dogs, by referring them to four different origins, be adopted, we may then conjecture that each of the four animals above named brought forth its own puppies, to be the progenitors of their respective families.

Let this doctrine of the new creation of new species, by generative development through variation be accepted, and we have Darwin's theory of the origin of species by successive generation; and instead of opposing the theory of Agassiz, it confirms it; because it adopts and reasserts the principle of new creations, and offers some explanation of the way in which they were made.

Let us glance—and only glance—at some facts in geology and zoology, to see what would be the effect of this principle; and I shall carefully limit myself to the most general suggestions, on a topic which would fill more than a volume.

At the beginning of the fossil records of life, in the Silurian formation, we find trilobites of various forms; and recently a Limulus or something akin to a Limulus has been found there. There are other Crustacea; but with these two only, is it not possible to account for all the Crustacea which have ever existed or now exist, without overstepping the rationally possible limits of extreme variation in offspring, simply by arranging those which we already know in a chain of affinity?

But how shall we get to the vertebrates? These same trilobites ran up through all the palæozoic rocks, through the Silurian, the Old Red-sandstone, the Carboniferous and the Devonian, and are lost at last in the Permian. Near their end, when they are already thinning out, we have, in the old red-sandstone formation, the "buckler head,"—or, to use the Greek name given by Agassiz, the Cephalaspis. And we have also the fossil flying fish, or using again the Greek name, Pterichthys. The first of these was long regarded as a trilobite of the genus Asaphus, until Agassiz

at length determined it to be a fish. Of the second, Murchison says in a letter to Miller, "if not fishes they approach more closely to crustaceans than to any other class. I conceive, however, that Agassiz will pronounce them to be fishes, which together with the curious genus Cephalaspis *form the connecting links* between crustaceans and fishes." Now, is it too much to infer from these facts, and always within the reasonable limits of generative aberration, that either of these animals, if a crustacean was so nearly a fish that some of its ova may have become fishes; or if itself a fish was so nearly a crustacean, that it may have been born from the ovum of a crustacean? We may add indeed, that the Eurypterus, now called a crustacean, was regarded at one time, by Agassiz, as a fish.

If fishes may thus have begun to be, and we may suppose that, having begun, they could be so arranged by their affinity and gradual difference as to account in this way for the successive new creation of their kinds, we may then pass to the question of reptiles.

Here also we have Lyell's Dendrerpeton, Owen's Placodus, and the Archegosaurus of von Meyer, all of which were held, and somewhat firmly held, by the highest living authority on this point—Agassiz again—as fishes; and all of them after further and final investigation, have been lifted out of the water by the same strong hand, and placed upon dry land, as reptiles. I know the explanation of this; but does not the fact itself suggest irresistibly that we have here what Murchison calls "connecting links." Links, that is to say, through which, by generative variation, the fish passed into the reptile, and so the family of reptiles began. So too, possibly, the Pterichthys, or fossil flying fish, the Pterodactyle, or huge winged fossil reptile, may suggest the possibility of a similar origin for birds.

As to that difference between vegetables and animals, which some have regarded as the greatest difficulty, I would say only what every one who owns or uses a microscope knows, that the line which separates the protophyta from the protozoa is constantly changing and always uncertain; and that if the organisms which lie along this line, should have offspring which are certainly vegetable, or those which are certainly animal, in neither case would the offspring differ much from the parent.

Nor let it be said that the geological records exhibit numerous instances where a race which succeeds another, does not come into existence until a certain period after the kindred race from whom they might have come has utterly perished. It is not quite so. On the contrary, in most cases, the great classes of animals lap over, as in the instances given, of crustaceous trilobites and the fish found with them, and again the fish and the earliest reptiles, in a way which has always suggested, of itself, this idea of succession by generative reproduction. There are eminent naturalists who read in the records of geology the plain declaration

that there have been some—perhaps many—cataclysmic destructions of whole orders of being, followed by periods characterized by the absence of organic life. If this were *proved*, there must have been not only many new *creations*, but many new *beginnings* of organic life. It must be remembered however, that the geologic record is assuredly not yet wholly unrolled; and that we are not sure that we read aright all that is seen. I have some doubts whether there be an instance in which such an interval of absolute nothingness unquestionably occurs; or one, even in the present state of our knowledge, in which among the races passing away there are not found, and far within the limits of extreme aberration, some who *may* have been their offspring, and the parents of succeeding races.

But I must forbear following these suggestions further. The difficulty of admitting the transformation is, I know, great; and still greater difficulties must be encountered in other parts of this supposed chain of reproduction. A very great one to my own mind arises from those beds below the Silurian, which, on the one hand, are wholly free from traces of life, and on the other, from evidence of destructive alteration by heat. They seem to me to lead strongly to the conclusion of Murchison and others, that the earth had only then become cool enough to make life possible, and consequently that life must have begun there; and there certainly we find it already very various. But, not to insist that with farther knowledge, wider discovery of "connecting links," or transitional forms, and better examination, all these difficulties may be materially lessened, I say at once that I should accept them all unhesitatingly, rather than the notion that the first horse, or dog, or eagle, or whale, flashed into being out of nothingness, or out of a mass of inorganic elements which had been drawn together in due proportion for that purpose.

This last supposition is inevitable if we reject the first.

The one thing I would be understood to assert, is, that science must now *elect* between two hypotheses, which together fill the whole ground, and cannot *both* be rejected. One is, that the animals and vegetables of the world have been formed, by absolute fiat, out of a mass of inorganic materials. The other, that they have come into being successively, by generative production, of some kind and in some way. When Milton tells us that

> * * * * The earth obeyed, and straight
> Opening her fertile womb, teemed at a birth
> Innumerous living creatures, perfect forms
> Limbed and full grown. Out of the ground arose
> As from his lair, the wild beast where he dwells
> In forest wilds, in thicket, brake or den.
> The grassy clods now calved; now half appeared
> The tawny lion, pawing to set free
> His hinder parts: * * * *

he adopts and adorns the first hypothesis; but while Milton was a great poet, he was not so great a zoologist.

I do not now assert that no creature *can* be made out of nothing, or out of the dust of the earth, nor do I speak of the first beginning of creation; nor of anything but the existing and extinct floræ and faunæ. In reference to the various species of these, I say only that this is the last conclusion which we should adopt, and only when driven to it. Perhaps I may illustrate my meaning thus. If a pair of undescribed mammals, about as large, we will say, as a fox, with young or preparing for them, were found to-day in some district in England which has been thoroughly explored, and of which the fauna and flora were perfectly well known, and these animals differed in some specific essentials from any known animal, there would be a vast amount of speculation about their origin. One writer would say that they had escaped from a menagerie or from some ship; another that they had always been overlooked and undescribed until now; another that they were hybrids, and there would be much discussion as to what animals could have produced them, like that which Gilbert White tells of about the bird which he thought a cross between a pheasant and a hen. There would be no limit to the extent or variety of the discussion,—excepting this. No naturalist would, I think, explain their appearance at that time and place, by supposing that they had been made out of nothing, or out of the dust, suddenly, where they were found. If any one ventured upon this hypothesis, I do not believe that it would be generally adopted. I do but apply the same way of thinking to past times. When the new species appears first in the geological strata, I say that its creation from nothing or from the dust should not be held, until all other possibilities of production are exhausted and rejected. For creation from nothing is just as possible now as it ever was; and we have no reason for saying that it would not be as natural *now*, as likely to occur, and as worthy of admission and belief.

What do we gain by the use, in this connection, of the word miracle in the sense of an exceptional interference by omnipotence? When one of the wheels of Babbage's calculating machine turns up its numbers in a certain unbroken series for a million of times, and then a new element is suddenly introduced, and an old one goes out, this apparently disturbing thing is just as much a part of the machine and its operation as all the rest. The illustration fails so far as this. Babbage calculates his machine and sets it going, and leaves its working to the natural laws which he finds in operation. God never leaves his machine, for if he did it would instantly perish, because it is always his present activity which gives force and efficacy to the laws by which He works.

But what shall we do with that other principle of Agassiz, that all this successive production or creation of new creatures has happened by the will of a creating God; or, to use his own

phrase, that each new creature has come into being by the fiat of the Almighty? What I do with it, is to accept it readily and entirely. For when the voice of God issues the fiat and says let this thing be, is it not as perfectly obeyed although that thing comes into being by generative development, as if it sprang forth from nothing or from the dust?

And again what shall we do with the principle of Agassiz, that in all these new creatures there is no chance and nothing arbitrary, but a coherence and coördination of parts, and a unity of purpose and of place, which prove the whole to be the work of one directing mind and one causative power. Again I answer, admit this also freely and gladly; thankful for every argument and illustration which enforce it. For what is there in the supposition that God has his own laws of divine order, and operates through these laws, and by the means which He has provided, (no matter how universal these laws or how far back the chain of influences or causes extends,) to prevent our recognition of Him and of his wisdom in his works.

But what shall we do on the other hand with Darwin's "struggle for life," and consequent "natural selection," which plays so great a part in his theory? Again I say, if farther investigation renders it probable, as I think it will, admit this also with perfect readiness to play whatever part sufficient evidence may assign to it, be that more or less. The fact *to some extent* is obvious and certain. And may not God act as well through this "struggle for life" as through any other of his laws? Must it be regarded as a blot, an imperfection, which he could not help, and bears with as he may? If we regard it as an instrument, by means of which he works out universal, inevitable, and never ending improvement, incorporating this law with the nature and essence of every thing that lives, or can live, may we not see in this also, at once his infinite love and his infinite wisdom?

Then as to hybridism. Darwin admits the vast preponderance of authority against the continued fertility of hybrids, but still thinks that there are some qualifications. Even since his book was published, Isidore St. Hilaire, who has made hybridism a special study, has published a work in which he asserts, and goes far to prove, that hybrids are sometimes at least just as fertile as their parents. Out of this uncertainty, let us draw one certainty; and it is that nothing is certainly known about it. And also one probability—that offspring may differ from their parents and brethren so very much that there can be no sexual intercourse between them. They may differ less and then there may be intercourse but it will not be productive. They may differ still less, and it may be productive, but the offspring will not reproduce. Still less and they will reproduce, but only for a few steps. Still less, and they will be as fertile as their parents or brethren. Scientific men may give to these degrees of differ-

ence the names of classes, of genera, or species, or what else they will. For here I will venture to remark that much of the criticism and discussion to which Darwin's work has given rise, both in England and in this country, seems to me verbal only. That is, it relates not to the origin and nature of certain existences, but to the language we should employ in speaking of them. What do we gain in real knowledge, when we insist that the word "species" *must* mean this or that, when it *may* mean anything, and very few persons use it in the same sense, or in any definite sense. And as to the question of difference or identity, do we know enough about it to be very positive on any point, except our ignorance? For how many years has the Tertiary formation been arranged into four classes—the Eocene with its one shell in twenty-five now living, the Miocene one in six, the Pleiocene one in two and a half, and the Pleistocene nine out every ten. DesHayes, a great man, has devoted himself to their examination, and has reasserted this with the most emphatic distinctness and the most abundant illustration: and Agassiz now comes and declares it to be all a great mistake. He doubts whether any one shell of the 4 per cent, the 17, the 40, or the 90, has ever been looked upon alive by man.

Far be it from me to undertake to decide between such men. But again let me draw one conclusion, which seems certain; and it is that there is no sure, unerring, and unmistakeable test of specific identity or difference.

If we admit with the qualification and in the way above stated the theory of the production of all things by generative development, and the active operation of this principle of the "struggle for life," and admit also Agassiz's requirement of new creations, and of the orderly succession and coördination of these, we have a theory composed of elements which certainly do not now oppose and destroy each other, but coëxist in harmony, and in mutual support and illustration.

How far shall we carry it? Not to the creation of all things from one beginning, unless farther investigations should remove the immense difficulties which this theory must now encounter, and sustain its probability. But let not the investigation be clouded, obstructed and defeated by the assertion that any theory which calls into being all existing and extinct organisms by some method of successive generative development, cannot be true, and must needs be false and dangerous.

The great difficulty to most minds would be, after all, that which relates to man himself. Man, from a monad! Yet let it not be forgotten, that this is the natural history of every man that has ever been born of woman. At first a nucleated cell, (call it a monad if you like,) not distinguishable from other nucleated cells, which, by segmentation, gives rise to that germinal membrane, from the outer portion of which are formed the or-

gans of animal life, and from the inner those of organic (or vegetative) life; and then, in its uterine development, exhibiting successively resemblances, more or less close, to the lower animals; the human embryo, for example, having, about the twenty-fifth day, the branchial openings and elongated body of a fish, at a later period the imperfect limbs of a seal, and still later the bent limbs of a quadruped. These, and many analogous particulars in the history of the human embryo, make this one of the most inexplicable and yet suggestive wonders of existence. One might well imagine that the "monad" retraces his footsteps along that immeasurable pathway from primeval being, and as it repeats, records them.

While all this is nothing like *proof* that man is also a product of this law of generative devlopment through variation, it may have some tendency to lead the mind in that direction. And how much there is elsewhere in the metamorphoses of nature, to exert upon the mind a similar influence. Tell one who is eating a ripe peach, and after enjoying all the pulp, breaks his teeth against the stone, and being curiously inclined opens that, and finds the solid meat, and opens that again, and puts the infolded plumule under a lens, and sees there the promise of a future tree,—tell him that skin, and stone, and seed and plumule, all are but *changed peach leaves*, will he not be at least as much surprised as if you carried him to a menagerie, and pointing to a hyena, said to him, there stands the father of the "yaller dog" of New England? *

But this notion of man being born from an animal stands in

* I allude, of course, to the January number of the Atlantic Monthly, wherein this strange animal is presented with that wonderful power of word-painting, which is a true daguerreotyping by the sunlight of genius.

But I write this note rather to refer to an article in the North American Review for July, 1857, in which Dr. Holmes, before the controversy about "Darwinism" began, treats many of the topics to which it has given rise, and exhibits his own views of an ever *immanent* God.

No one can admit more cordially than I do, the principle which has been recently so much considered, that God must have had at and from the very beginning of his action, laws, to which he and his universe have always, and, I am willing to say, necessarily conformed. So too, I admit, as cordially, that other principle, that all science, philosophy and reason, lead concurrently to the conclusion, that the "Causa causans" must be always and incessantly a present cause, as *present* at one period of duration as at another, and always directly and universally operative. But why regard these principles as antagonistic? To me they seem not only harmonious, but complementary, and necessary each to the other. If I believe that God is ever present, active and operative, it is because I believe that the laws of order which arise from his own divine nature, permit and require this. If I believe that these laws exist, that he has ever conformed to them and must ever do so, it is because I believe that they are the eternal instruments of his ever active love and wisdom. In the words in which Dr. Holmes sums up the whole matter at the close of his article in the N. A. Review, "whatever part may be assigned to the physical forces in the production and phenomena of life, all being is not the less one perpetual miracle, in which the Infinite Creator, acting through what we often call secondary causes, is himself the moving principle of the universe he first framed and never ceases to sustain."

the way of positive revelation! In my own mind it does not. I look upon the Bible as the word of God: but I do not believe that the first chapters of Genesis teach or were ever intended to teach natural scientific truth; nor does this denial lessen my reverence for what I consider as the moral, spiritual, and religious truth which I believe they do teach directly, or under the form of parable and symbol. And upon the question of the original and physical creation of man, I think that we know no more and no less, and are at equal liberty to think, to argue, and to conclude, as if these chapters had never been written. To me, they do not say one word about it.

But does not this notion stand in utter opposition to all religious belief? Again, I can only say that in my own mind it does not. I believe, most unreservedly and undoubtingly, that man is superior, not in kind only but in degree, to all animals, and is immortal, which they are not. But this belief would not be either shaken or troubled, if science should, upon evidence discovered hereafter, teach, that the Gorilla, which Owen says is most like to man, or the Chimpanzee, which Professor Wyman, with better reason, places higher,—if either or both had given birth, when the fit time had come, to a babe, whose brain and nervous system, with all the residue of its frame, were so organized that the breath of life, of spiritual and immortal life, could be breathed into him, and bear with it all the attributes of human nature,—all those attributes which divide, as by an unfathomable abyss, the man from the beasts that perish, and lift him infinitely above them. At present, science possesses not only no facts which would lead to this as a certain conclusion, but none which would declare it to be a probability. But neither has it sufficient reason for asserting it to be an impossibility. Nor, does it seem to me, that religion would receive a blow, if science should be led by additional discovery and more thorough investigation, to go not only thus far, but so much farther, as to account for the various kinds of men by asserting that the brown oran-outang that lives among the brown Malays was their progenitor; the black gorilla the father of the black races, among which he is still found; other simiæ the parents of other human families; and some one fairer than the rest, the remote ancestor of the Circassians, whose superiority over their progenitors was so great that they had rooted him out from the earth!

But let us consider the general relation of this hypothesis to religion. I am perfectly willing to confess that the theory propounded by Darwin, as it rests upon excessively minute changes, and those produced by what he calls "accident," (of which word, however, and of his use of it, he offers much explanation) seemed to me to have a tendency to obscure the thought of providential causation and government; and that I was first led to reason out, as well as I could, the probability and effect of more salient changes in the offspring, by its appearing to open the door

to this thought somewhat more widely. But aside from this, and indeed from any reference to this or any special question or theory, may it not be well to remember, that natural science belongs, mainly at least, to the intelligence of man, and to his outer life, while religion belongs, mainly again, to his affections, his motives and his inner life. Hence, entirely different faculties and functions of our common nature are brought into exercise in reference to science, from those which are invoked by religion. It is a good and wholesome thing for a man to become religious because he chooses to be so, and loves to be so; and it is good for him to compel himself to make this choice. He cannot indeed become religious on any other ground or in any other way. And therefore Divine Providence has mercifully guarded him, not only from the external compulsion, which, as all men see, cannot reach the heart, but from the compulsion of his own intelligence, which might be equally injurious.

In investigating the claims of science, he must call upon his intellect to look sharply at the facts, the logic, the arguments and the conclusions; and this is all, or nearly all. But he must choose and hold his faith, not by means which logic disdains and denies, but by asking of logic to do all that it can do and and the best that it can do, as the instrument of something higher than itself, which can take up and complete the work which mere logic must leave unfinished.

How easily could God have written his word and his truth in fire upon the sky, and in gold upon every leaf or stone, if all he had desired was the intellectual advancement of man. We may infer from his course of providence, that he desires this, only as a means to an end; and as an instrument of that moral and affectional improvement, which must be man's own coöperative work. Therefore it is, that religion never has been, and I think never will be fortified by the demonstrations which belong to ascertained science; and hence it is also that no science, and no mere truth has ever yet been suffered to arise on the world, and none I think ever will be, that does not leave man free to be irreligious if he will; although all true science offers him much to feed upon and to rejoice in, if he loves to look upon the truth he learns as aliment for his religion.

To every creature is given a tendency and a capacity to seek and find and appropriate that food which agrees with its own nature. When a willow tree sends a root far in one direction to a ditch where it may drink its fill, and a neighboring grape vine sends its root as far in an opposite direction and finds a heap of buried bones, we have but the operation of the same law, by virtue of which if ten men read a book, it may be to them ten books; for each will read the same words, and then translate them in his own way. It is an old saying, that what one brings home from foreign travel, depends upon what he car-

ries with him. So it is in the journeyings of the mind. Let that go where it will it carries itself, and uses itself as the organ for giving form and effect to all that it receives.

The poet may say that the undevout astronomer is mad; but astronomy, and every science cultivated among men, has those who are devoted to it with the most faithful assiduity, and who extend its borders and enlighten its dark places, and who are, nevertheless, utter unbelievers as to God and religion; and find in their science support for their unbelief. To minister to religion is the highest, the consummating work of science; but science cannot render this service where there is no religion to accept it. So will it be with the theory of the creation of all things by successive generative and variant production, if it be established in any form whatever.

This man will read it to whom the idea of God is an offense and a pain. His unbelief holds him in subjection; and when he reads any book, or studies any subject, he reads with clouded eye and mind all that favors religious truth, but brightens at once when he gets a fact or an argument for his unbelief, and dwells on that as a choice morsel. He will study this new theory, and find in it new evidence that God is a mere superfluity; and he will say exultingly, now we have proof that the laws of the world and their own necessity are all that a truly rational mind can ask. And he will deny, or forget, that there is no possible conception which so imperatively demands a lawgiver, as law; and none which so requires a cause to set it in action, as an active necessity.

Another man who loves to believe that God forms and fills and *is* the universe, and that there is no other God, will find here abundant support for his opinion, and will rejoice in the evidence this theory affords of the universality of law and the connection of all things by gradation into unity. And he will forget, or will not know, that all this implies design, and purpose, and will, and therefore personality.

And a third man will see in this theory new proof of the eternal working of the personal God in whom he believes. He will rejoice at the evidence it offers that God loves to bless every entity of his creation by using it as his own instrument and as the means for farther creation; that preservation is continual creation; and that he forever puts forth the same power, born of the same love and guided by the same wisdom, that in the beginning laid the foundation of the universe deep in that infinite which no plummet of human imagination ever can sound. To such a mind it will be a new proof, that from God's own nature, there came forth laws of order, in which, through which, and by which, he has ever worked, from a beginning, which when we try to think of it, recedes faster than thought can follow.

Cambridge, May, 1860.

ART. II.—*Notes on the Habits of the Common Cane,* (Arundinaria macrosperma, *Michx.*); by HUGH M. NEISLER, Corresponding Member of the Essex Institute, Salem, Mass.

THE common cane, as it springs from the seed, bears no slight resemblance to some of our coarser and more reedy Panicums; rising, during the season of its growth, according to the more or less favorable circumstances of its locality, from the height of a few inches to that of several feet, forming a straight unbranched culm, with a bud at every node. The culm itself completes its entire growth the first season, and though it endure for years, even to the time of its flowering and fruiting, never afterwards increases in height, only becoming tougher and stronger by the gradual deposition of siliceous matter on its surface, and ligneous matter in its substance.

The second year, circumstances being favorable, its lateral buds are developed—forming slender, upright branches—whose growth in length likewise terminates with the season, and which rarely grow much higher than the summit of the culm.

The third year, the buds of these lateral branches are developed, and they, in their turn, form other lateral branches with their buds, and thus the plant continues to grow until it flowers and fruits, until its existence is terminated by accident, or it becomes so much crowded that there is no longer room for its growth in this manner. However, the buds of one season do not invariably put forth the next; but, if circumstances are unfavorable to their development, they may remain dormant for a time, and then put forth or eventually perish.

The first year, besides the growth already described, the plant throws off one or more subterranean culms, popularly termed "chain roots," differing from the others only in being white, with very short internodes, and these clothed with imperfect sheaths, or naked, the sheaths becoming obsolete or, as is more commonly the case, reduced to a circle of rootlets around the node. From the buds of these subterranean culms springs the second year's growth of cane. This is subject to the same laws and grows in the same manner as that of the first year, likewise throwing off its subterranean culms, from the buds of which springs the cane of the next season, and so on it proceeds for years, new cane coming up each successive season, until it is so crowded there is no room for more to grow. The buds of these subterranean culms may, like the others, be developed the year after they are formed, or remain dormant until circumstances favor their growth.

The cane of each succeeding season becomes stouter and taller than that of the preceding; and as the cane of one year is overtopped and shaded by that which grows up the next, it dies out gradually and gives place to the larger cane of some future season; hence arises the great uniformity in size, observable generally in the plants of a cane-brake.

It may not be amiss, in this place, to observe, that the cane is described by authors as "branching towards the summit"; this is correct as applied to plants growing in a crowded cane-brake, where the development of the lateral buds is prevented; but where it is uncrowded and free to grow, it presents itself, clothed from the ground upwards, with numerous erect, closely appressed branches, as the seedling plant is described near the beginning of this article.

After the growth of a number of years, and when the cane has reached as great a size as the circumstances of the locality will admit of, it springs up only in such places as may yet be unoccupied, until the whole brake becomes as thickly crowded as possible. After arriving at this point, its growth altogether ceases, excepting from the few buds that may be formed at the summit, and may chance to be freely exposed to the light and air. In this condition it will remain for a series of years, until the time of flowering arrives. If, however, whilst in this condition, any or all of it by chance be destroyed during the winter, its place will be supplied by cane of equal size the following season, from the buds of the subterranean culms; but if an accident of this kind happens during the growing season, it never grows up again, the whole plant dying; though if any escape the accident, the extension of their "chain roots" may gradually fill up the vacancies left by the plants that have perished.

The size which the cane ultimately attains is as various as the soil in which it grows. In places peculiarly unfavorable, it may not be higher than six inches; in the "Piney-woods" swamps, in my immediate vicinity, ten or twelve feet is about the average height. On Flint river it is common to find it as high as twenty-five and thirty feet; whilst I have seen occasional specimens cut from the swamps of the Uchee in Alabama, which, though not actually measured, I should judge, could not have fallen far short of forty feet. In the same cane-brake, too, if covering ground which presents striking diversities of soil, it will be found to vary greatly in size.

We have above remarked that when cane reaches its full size, it remains almost unchanged until the time of flowering: how long this may be I have not the means of determining. It is evidently many years. A piece of land, many acres in extent, was once pointed out to me in the low grounds of Flint river, by a gentleman residing in the vicinity, who informed me that on his removal to that country, it was covered by a dense growth of large and full-grown cane, and that it flowered and fruited fifteen years thereafter. If in this instance we suppose it to have required ten years to bring it to the condition in which he first observed it, its time of flowering would be brought to twenty-five years—the age at which, in this region, it is pop-

ularly supposed to flower and fruit. This, however, is but a supposition.

When the flowering once begins, it takes place in every plant in the cane-brake at the same time, whatever may be its age, its puberty depending not on the actual age of the cane itself, but upon the time that has elapsed since the germination of the seed from which it has either directly or indirectly sprung. When the time comes, it flowers, whether it has grown up from the ground ten or twenty or more years before, or whether it has yet to complete the first season's growth. In the spring of 1857 I found a small field from which the cane had been cut the preceding winter. The husbandman had been so careless in clearing out the young cane that had come up amongst his corn, that great numbers of them were left growing about. They were generally about three-fourths of an inch in diameter, but only some five feet high, and still so tender and soft that they could be easily crushed between the finger and thumb to within one and two joints of the ground; yet every plant was in full bloom. There the time of flowering had overtaken it when little more than two months old. It is, however, no uncommon thing to find individual plants in bloom, sometimes several years before the general flowering commences. This is, however, only an exception, an occasional precocity brought about by some extraordinary cause.

With the ripening of the seed ends the life of the plant. It then perishes root and branch, as entirely as an annual grass; and if the same ground be ever occupied by cane again, it must be produced anew from seed. Authors, I judge, have been unaware of this fact, from their describing the plant as fruiting at stated periods, or at more or less distant intervals; and this error perhaps may have originated from their noting the times of the flowering of the plant, as it occurred in different localities; or, if observed in the same place, the intervals have been so long between the times of flowering, that the fact of its having been in the meantime reproduced from seed, may have escaped recollection.

The question here presents itself, Whether there are two species or varieties, or but one? Within the comparatively narrow limits to which my own researches have been confined, I have had opportunities of gathering the flowers of both large and small canes; but specimens have yet to be collected that present a difference. As to the question in general, if the reader bears in mind, that the cane in the earlier years after the germination of the seed is much smaller than that which follows at a more distant day, he will readily see, that if both should chance to survive for years, they might appear so different as to be regarded as distinct varieties, if not species, especially as the flowers are rarely procurable to prove their identity. The same may be said of the differences produced by differences in the character of the soil in which they grow.

Taylor Co., Geo., 1860.

ART. III.—*Experiments on the forms of Elongated Projectiles;* by OGDEN N. ROOD, Prof. of Chemistry in the Troy University.

ALTHOUGH during the last few years great attention has been paid in Europe to the improvement of rifle projectiles, yet much obscurity seems still to prevail on this subject, as is sufficiently evinced, by the different and opposite courses pursued by the several governments; the variety in the shapes of the balls employed at the present time being almost as great as that in the patterns furnished by a kaleidoscope.

The inquirer in this department meets with very *general* answers, and is too often told in substance, that the accuracy of performance is directly proportional to the excellence of the ball's model; it being in the meanwhile by no means particularly apparent in what such excellence consists.

An explanation is found in the fact, that there is perhaps no other field of investigation, in which so great a number of experiments is essential to the establishment of single and even isolated facts—wind and weather, slight changes of temperature in the tools employed, or in the form and fit of the projectiles, as well as other and more obscure causes, all largely influencing the results, contribute and combine vastly to complicate what at first glance might seem a moderately simple problem.

A series of experiments lately instituted by me, had for their object the examination of a few of the more obvious considerations relative to the accurate flight of elongated projectiles, such as length, the form of the base, and other points.

The rifles employed, were, with the exception of No. 5, made by Nelson Lewis, of Troy, N. Y., and were of the model sometimes called "Kentucky," or more properly, "Improved American."*

Our marksmen know the quality of Mr. Lewis's work, although it may be well to state, once for all, that rifles of this model as manufactured by him, and a few other makers in the United States, hold much the same relation to the English or Continental arms that the Oertling balance does to the scales of the apothecary; a fact which has not escaped the notice of the author of the article on Gun Making, in the Encyclopædia Britannica, 1856, vol. viii, p. 101, where, after a description of this rifle as made by James, of Utica, N. Y., and an account of its performance at 220 yds., he concludes: "The whole of the ten shots would have gone into a small sized playing-card. A feat of this kind is probably unparalleled in Great Britain, and it may draw

* For some account of these rifles see an excellent article published in the Atlantic Monthly for October, 1859.

the attention of our own makers to the propriety of diminishing the calibre, and increasing the speed of the ball."

Dimensions of the rifles employed.

	Length of Barrel.	Weight.	Calibre.	Twist of grooves gaining, ending in one turn in inches.
		lbs. oz.		
No. 1.	30·5 in.	16 0	0·46 in.	
No. 2.	31·0 "	9 0	0·34 "	42·35
No. 3.	30·4 "	7 4	0·42 "	
No. 4.	12·0 "	2 12	0·36 "	47·37
No. 5.	5·5 "	0 8	0·20 "	12·85

They were all provided with the sights known as the "Globe and Bead," as well as with set locks. Nos. 1 and 2 were supplied with the false or loading muzzle, and all with the exception of No. 5 had "guide-starters," which ensured the accurate placing of the ball in the barrel. Linen patches were in every case employed, not greased, but moistened, it being now pretty well understood that the latter afford the more accurate practice. As a general thing, after each shot the barrel was wiped out with a slightly moistened rag, dampness being guarded against by a repetition of the process with one that was dry.

Length of the Projectile.—As has been intimated, great variety of opinion seems to exist as to the proportion which the length of the ball should bear to its diameter measured at the base, balls being used in Europe whose length ranges from 1·36 to 3 diameters. The advantages of length are generally known and there are writers who advocate progress in this direction, imagining naturally enough that a ball modeled after the lines of a ship would experience less resistance in passing through the air than its shorter and blunter rival. Some months ago, filled with this idea, I constructed a ball of the model shown in Fig. 1, sharp both at bow and stern, and having of course its centre of gravity farther forward than is usually the case with solid projectiles. Its penetrating power at short distances was great, but its complete and total failure otherwise, resulting from the impossibility of communicating a sufficiently high rotary motion, led to an examination of the rates at which it is necessary for balls of different lengths to revolve in order to secure accuracy of flight.

1.

Diam. ·36 in.
Length 1·27.
3·53 Diameters long.
Weight 222 grs.

Now it is evident that if the velocity with which any ball leaves the rifle-barrel be known, and likewise the twist of the grooves, the rate of revolution of such ball per second is easily found; thus, if the initial velocity should be 1000 feet per second and the twist one

turn in 5 feet, it is plain that at the moment of leaving the muzzle, the ball will be revolving at the rate of 200 times per second.

To ascertain then the initial velocity, I constructed two ballistic pendulums, similar to those used by Robins. The total weight of the larger pendulum was 21·75 lb., and it happened, by chance, that 120 of its small swings were performed in two minutes; consequently its centre of oscillation was distant 39·13 inches from its axis of suspension: the centre of the pendulum was 39·7 inches from the axis, while the centre of gravity was 37·4 inches distant from the same point: therefore we have,

$$\left.\begin{matrix} 39{\cdot}7 : 39{\cdot}139 \\ 39{\cdot}7 : 37{\cdot}4 \end{matrix}\right\} :: 21{\cdot}75 : 20{\cdot}2 \text{ lb.}$$

or the pendulum resisted the impact of a body in motion as though its weight had been only 20·2 lb. The axis was made of a bar of steel, two sides of which were ground to an edge. Instead of the ribbon used by Robins to measure the chords, a narrow strip of paper was employed, which was fastened to one side of the pendulum on a line with its centre, that is 39·7 inches from the axis.

The smaller pendulum was similar in construction: its weight was 7833 grs., it made 93 oscillations in one minute, consequently its centre of oscillation was 16·29 inches distant from the axis; the centre of the pendulum and its centre of gravity were distant from the axis 16·6 and 15·2 inches; it resisted therefore as though its weight had been 7038·4 grains.

For barrel No. 4 I succeeded in constructing, after many trials a ball or picket, on the American model, Fig. 2, whose flight was accurate up to 500 yards; its dimensions are given below. This ball was fired with different initial velocities at targets of *pasteboard*, placed at certain distances from the rifle; an examination of the holes made, at once indicated whether the rate of rotation sufficed to compel it to fly truly and point foremost; for if this were not the case, the picket of course, made *oval* holes or struck the target sideways.

2.

Ball No. 4.
1·67 Diam. long.

Ball No. 4.

Length ·62 in. Diameter ·37 in. Length in diameters, 1·67. Weight 113·34 grains.

Initial velocity.	No. revolutions per sec.	Distance.	Point foremost.	Oval.	Sidewise.
520 ft.	131·7	20 ft.	1	4	6
520 "		36 "		2	2
691 "	175	20 "	3	1	1
691 "		36 "		2	2
691 "		165 "			8
849 "	215	75 "	1	1	2
849 "		165 "	2	1	2
965 "	244	165 "	8	6	3
1128 "	285·8	1500 "	Flight accurate.		

It will be seen from these experiments that a ball of the above model, and 1·67 diameters in length, must make 280 revolutions per second to ensure its accurate flight, when discharged with an initial velocity of 1100 feet per second; what rate of revolution would be necessary to sustain it point foremost, were the initial velocity different, is another question, and one which will presently be noticed.

Let us examine the case of a ball slightly *longer* than the above. Rifle No. 2 was now employed, its ball was slightly blunter than No. 4; it had often made excellent practice at 220 and 500 yards.

Ball No. 2.

1·77 diameters in length. Weight 105·01 grains.

Initial Velocity.	No. Rev.	Distance.	Point foremost.	Sidewise
1051	298	165 ft.		all
1199	340	660 "	all	

By a comparison with the last table, it will be seen that this slight increase of the length has carried with it the necessity of 50 additional revolutions per second. The lowest initial velocity I have ever actually employed in this rifle for target practice was 1774 feet, the highest 1917 feet, so that the number of revolutions made by the ball in actual practice was either 503 or 543.

Balls 2 *and* 2·5 *diameters in length.*

A longer ball for barrel No. 4 was now constructed: its weight was 141·5 grains, diameter ·36 in., length ·74, so that it was in fact 2·05 diameters in length. The following results were then obtained:

Ball 2·05 *diameters long.*

Initial Velocity.	No. of Rev.	Distance.	
1063	269	18 ft. 42 ft.	all sidewise.

The above shows that with this model 269 revolutions are altogether insufficient, if the initial velocity be as high as 1063 ft.

The length of the ball used in the Swiss federal rifle is 1·0039 inches, its diameter 0·41 inches, or it is 2·44 diameters in length, weight 257 grains, weight of the charge 62 grains. Making use of the British empirical formula $V = 1600\sqrt{\frac{a\,p}{w}}$, we have the initial velocity $= 1600\sqrt{\frac{3\,62}{257}} = 1361$ ft., and as the twist is one turn in three feet the ball leaves the barrel making 453 revolutions per second.

The initial velocity in Jacobs' rifle, judging from his statements, must be about 986 feet: as the twist is one turn in 28 inches, the ball makes 422 revolutions per second.

Now why not, it is often asked, project either of these balls with an initial velocity of 1600 or 1700 feet per second? Simply because the above rates of revolution then become wholly insufficient. To test this point, I constructed for barrel No. 2 a ball weighing 136 grains and only 2·08 diameters in length, consequently not requiring as rapid a rotary motion as the Swiss ball.

Ball 2·08 *diameters in length.*

Initial Velocity.	No. of Rev.	Distance.	
1715 ft.	486	165 ft.	All sidewise.

It would probably require a rate of at least 600 revolutions per second, and the Swiss ball being again much longer would hardly fail to need a rate of from 700 to 800.

The twist of Whitworth's rifle (ball 3 diameters long) makes one turn in 20 inches: its initial velocity is not given, though it is understood to be greater than that of the Enfield: *if* it be as high as 1600 feet, the rate of revolution of the projectile will be 960 times in a second; if 1700 feet, 1020!

The above mentioned experiment will perhaps suffice to establish the fact that for any given projectile, the necessary rate of revolution increases rapidly with the augmentation of the initial velocity.

The inability to use larger charges of powder prevented me from pursuing the investigation farther in this direction, but it still remained possible to invert the order of proceeding and to examine whether at very *low* velocities the necessary rates of revolution for projectiles 2 and 2½ diameters long rapidly decreased. An elongated ball for barrel No. 5 was now constructed: its length was ·38 inches, diameter ·2, *i. e.* 1·9 diameters in length; weight 20·8 grains. The initial velocity was determined with the smaller pendulum and found to be 232·6 feet: the velocity was determined also from the time of its flight: a distance of 15·41 feet gave it 233: the average of 14 shots at 44·2 feet made it 216.

Ball 1·9 *diameters in length, weight* 20·8 *grains.*

Initial velocity.	No. of Rev.	Distance.	
232	216	75 ft.	Flight accurate.

Another ball was now constructed for this barrel; the results were as follows:

Ball 2·3 *diameters in length, weight* 26·5 *grains.*

Initial velocity.	No. of Rev.	Distance.	
202	188	75 ft.	Flight accurate.

A third ball, 3 diameters in length, was finally made for the same

A third ball, 3 diameters in length, was finally made for the same barrel: it was cast with inclined bands on it, extending for two-thirds of its length and corresponding to the grooves of the barrel:

Ball 3 diameters in length, weight 42·3 grains.

Initial velocity.	No. of Rev.	Distance.	
157·9 ft.	147·5	30 ft.	Flight accurate.

Patches were not used with these three balls.

As a termination to this portion of these experiments, I constructed the model seen in Fig. 3, 2 diameters in length and provided with a steel axis: it was caused to rotate by the thread on which it was suspended: small discs of paper divided into green and red sectors were attached, it having been previously ascertained by a revolving machine, with what rate of revolution each disc assumed a neutral tint. The model being made to rotate was allowed to fall from a height and it was found that six revolutions per second enabled it to remain point foremost in a fall of five feet, while 20 per second sufficed for a fall of 45 feet.

3.

The only question that now remains is, if 700 or 800 revolutions per second are necessary for the flight of a projectile 2½ diameters in length at a high velocity, why should this rate of rotation not be communicated to it? The answer is found in the practical difficulties; the friction and recoil become enormous, the act of discharge twists the rifle over sidewise and out of the line of sight. In the American Rifle, with a gaining twist, where the friction, twisting &c., are reduced to a minimum, it has been found that the maximum number of revolutions per second that can properly be communicated to a ball is from 500 to 550, and many will be ready to deny that even this rate can be employed with other than very heavy rifles of very small calibre without greatly impairing the accuracy. According to Chapman, a rifle having a regular twist of only one turn in four feet, calibre 80 round balls to the pound, charge of powder 2 inches to the bore, [initial velocity about 1680 feet, rate of rotation 420 times in a second] will, "when fired, twist over sidewise in spite of all you can do, and also kick or recoil very severely.* Surely if such recoil and twisting can be felt and seen, the tendency of the bullets to scatter and strike the target in a circle and not in a straight line is easily accounted for."

* It appears that European projectiles moving with a low velocity are subject in the longer ranges to a horizontal drift independent of the wind. This is attributed to the action of the grooves in the rifle, and has been found to correspond with their direction. No such deviation was observed by Gen. Jacobs (page 27 of Rifle Practice,) and he attributes it to the heavy recoil of the rifles used. As far as I know, it has never been observed in the American rifle where the recoil is very small, and the *trajectory very much flattened*, though it is the custom of our marksmen on calm days to practise at targets placed indifferently at 110 and 500 yards.

But where accuracy is aimed at, it is highly desirable not to employ a rate of revolution such as is able barely to cause the projectile to fly point foremost. A very notable excess above this quantity is essential for good practice.

One point remains to be noticed, viz.: the disfiguration which balls longer than 1·9 diameters suffer, when discharged with more than a very moderate velocity, in their passage through the necessarily strongly inclined grooves of the rifle. Gen. Jacobs states (page 23 Rifle Practice) that his projectile 2½ diameters in length performed admirably with a charge equal to one-fifteenth of the weight of the ball, but adds that he found, "A very slight increase of the charge caused the lead to change its shape under the pressure of the gunpowder so that a ball of the shape of Fig. 4 came out like Fig. 5. The lead was found totally incapable of preserving any resemblance to its original form under the pressure of a charge of powder equal to one-eighth the weight of the ball." From this dilemma the General escaped by making the forward part of the ball out of zinc; Whitworth provides for this difficulty by the use of pewter, which again necessitates a polygonal bore and a regular twist.

4 and 5.

Finally, during the last few years many of our best gunsmiths and marksmen have endeavored to increase slightly the length of the balls of their rifles above the standard dimensions given below, but in spite of the heavy charges used (3½ inches to the bore sometimes,) it does not appear that their efforts have been crowned with success, every fifth or sixth shot straying off some inches, or even, in the worst cases, striking the target sidewise at 220 yards.

It is perhaps hardly necessary to dwell farther on this matter of length, for while all the foregoing tends to explain why it has been found necessary in Europe to use low initial velocities with these *highly* elongated projectiles, it at the same time gives us little reason to expect that they will ever be able to compete with their shorter and more manageable rivals.

Below are the dimensions and weights of a number of balls belonging to first-class American rifles, each of which has been distinguished for its accurate practice:

Diameter of the Base.	Length.	Length in Diameters.	Weight in Grains
0·36 in.	0·62 in.	1·72	112·7
0·35 "	0·62 "	1·77	105·
0·46 "	0·82 "	1·78	243·6
0·47 "	0·87 "	1·83	249·
0·45 "	0·86 "	1·91	265·8
0·44 "	0·85 "	1·93	225·

But assuming the length of the projectile to be within the above standard American limits, it is still possible to increase its weight by adding matter about the point, which of course becomes blunter. A ball was constructed of the same length as No. 4, but weighing 121·1 instead of 113·6 grains, the $7\frac{8}{10}$ additional grains of lead being disposed about its point. As has before been seen, the old ball with an initial velocity of 1128 feet, and with 286 revolutions, had been accurate in its flight, but with the same charge of powder the new ball flew sidewise: the charge was increased; still with an initial velocity of 1443 feet, and making 365·5 revolutions per second, the new ball flew untruly, making oval holes in the target at 165 feet.

At present it seems to be the opinion of our best marksmen that a projectile constructed with a very moderately heavy point affords proportionately better practice at 500 than at 220 yards.

The Curve of the Projectile.—The bounding curve of the projectile has been largely experimented on in the United States, and has been slowly perfected by a class of men whose stock of mathematical knowledge was, and is, exceedingly limited: the results of their experimental labors are on this very account the more interesting.

I selected balls belonging to three excellent and well-tried rifles (by different makers) and placed them under the compound microscope, which was arranged so as to magnify only about five diameters: a camera lucida being attached, by its help the magnified images were transferred to paper. The curves were examined and found to be portions of ellipses, the main difference being in the points of the balls, which were slightly sharper. In two of the balls, with this exception, the coincidence was almost perfect: the curve of the first formed a portion of an ellipse whose diameters were in the proportion of 5 to 0·9; in the second the diameters were in the proportion of 5 to 1, in the third of 5 to 0·86. The agreement was pretty close with the third ball, but its bearings had been made more nearly parallel with its axis than was the case in the others, consequently here it differed slightly from an ellipse.

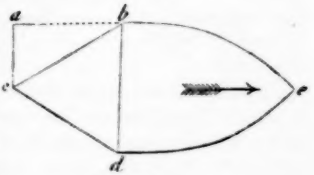
Form of the base of the Projectile.—The base of most balls now used is either flat, slightly convex as in the American model, or more or less deeply concave as in all those constructed on the expansion principle. It is generally admitted that balls having a flat base and moving with a velocity greater than 1200 feet per second, leave behind them a perfect vacuum, which, in addition to other sources of resistance, retards their progress by a pressure in front of 15 pounds to the square inch.

As the air is assumed to rush into a vacuum at the rate of 1150 feet per second, it now becomes a question whether the

base of the ball could be so shaped that in its passage through the air this vacuum might in great part be prevented by air rushing in at a certain, but lower rate than 1150 feet. It is evident that the lower this rate is made, the more do we subtract from the pressure in front, that is, diminish the resistance to the flight.

Suppose the base constructed like *b c d*, Fig. 6, and the velocity of translation 1100 feet per second, then it is evident that while *a b* is the measure of the velocity with which the point *b* moves from *a* to *b*, viz: 1100 feet, *a c* will be the measure of the velocity with which the air must rush in to prevent a vacuum, and by construction *a c* is only 635 feet.

6.



$bd = cd$
$bd \times 1.9 = ce$

We should be led to expect that a projectile of this shape would experience less resistance than one provided with a flat base, until the velocity of translation was increased so that *a c* became equal to 1150 feet, which in the model used by me takes place of course when *a b* or the velocity of translation is 1992 feet per second. Every effort was made to give these double cones as great a length as possible; many models were tried, and the length was slowly diminished until the new projectiles flew truly; finally, moulds for two balls of this model were perfected for rifles No. 2 and No. 4, and after some slight alterations the new double cones rivaled in accuracy of performance the pickets with flat bases. To accomplish this, it was found necessary to cut the patch as seen in Fig. 7, which ensured accurate loading.

7.

Patch for double cones.

Below are the results of a number of experiments on the time of flight of the old flat-ended balls and of the new double cones, together with their average velocities from 110 to 500 yards.

Single Cones. (Weight 113·34 grains.)

Rifle No. 4. Initial Velocity 1128 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·3323 sec.	993 feet.
220 "	·6947 "	950 "
500 "	1·885 "	795 "

Double Cones. (Weight 109 grains.)

Rifle No. 4. Initial Velocity 1138 ft.

Distance.	Time of Flight.	Average Velocity.
110 yards,	·32795 sec.	1006 feet.
220 "	·71725 "	920 "
500 "	2·05680 "	729 "

Single Cones. (Weight 105·01 grains.)

Rifle No. 2. Initial Velocity 1917 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4175	1580 feet.
500 "	1·3080	1146 "

Double Cones. (Weight 100·8 grains.)

Rifle No. 2. Initial Velocity 1867 ft.

Distance.	Time of Flight.	Average Velocity.
220 yards,	·4552	1449 feet.
500 "	1·4120	1062 "

It will be seen from these tables that the flight of the double is *slower* than that of the single cone, particularly at high velocities: this disproportion diminishes as we use lower velocities, but even here the new seems to possess no advantage over the old form—the sharpness of the stern not at all compensating for the necessary bluntness of the bows. It occured to me that this inferiority in the flight of the new balls might be owing to a disfiguration in their shape produced by the explosion of the powder: a number of them were accordingly fired into a bank of snow distant 500 yards: when dug out they were found to be as perfect in their proportions as at the time of loading.

It would seem that if the conical has any advantage over the flat base, it is only at velocities as low as 400 or 500 feet per second. These experiments also tend to show that the vacuum behind a projectile does not suddenly cease at a velocity of 1150 feet, but that its diminution is very gradual: they farther indicate to some extent the importance of making the forward part of the ball *sharp* when a high velocity is desired. Indeed, the air struck by the forward part of the ball seems to be thrown from it with such force, at high velocities, that a vacuum is produced behind it, whatever its form may be; and the vacuum is more complete the blunter the point is made. This may account fully for the disadvantage of the double cone.

It is well understood that the *weight* of the projectile exercises much influence on the time of its flight at the longer ranges: the time of flight of a ball weighing 105·01 grains and starting with a velocity 1917 feet has already been given: below are the results obtained with a heavier ball.

Rifle No. 1. *Weight of Ball* 243·6 *grains.*

Initial Velocity 1602 ft.*

Although the difference in the initial velocities was 315 feet, yet at 220 yards the difference in the average velocities was

* The initial velocity was *calculated* from determinations made with rifles Nos. 2 and 3. The determination from No. 2 when reduced gave it 1582·5 ft., that from No. 3, 1621·5 ft., the difference being only 39 ft.: the mean of these numbers is given above.

Distance.	Time of Flight	Average Velocity.
220 yards.	·477 sec.	1480 ft.
500 "	1·345 "	1115 "

only 100 feet, and at 500 yards it was reduced to 31 feet; showing that after a flight of some 600 yards the lighter projectile is overtaken by the heavier. It may be remarked here that the average velocity of these two rifles during a flight of from 500 to 600 yards, is as high or higher than the *initial* velocities of many European guns! Thus the French Tige Rifle has an initial velocity of 1023 feet, the Enfield Rifle 1115 feet, the Belgian chasseur carbine 1007 feet, United States new rifle musket 963 feet and the altered Harpers Ferry rifle 914 feet per second. Certainly Maj. Barnard is justified in his animadversions on the evils which have attended the introduction of the "shot-gun principle" into modern rifles; and with much reason he exclaims—"a decided step has been made *backwards* in losing that most essential element to range and accuracy, initial velocity."*

Position of the Centre of gravity.—In many European projectiles, every effort is made by hollowing out the base to throw the centre of gravity "well forward," in order that the disposition of the ball to fly point-foremost may be encouraged as much as possible, and for the furtherance of the same desirable end, grooves are usually made about its cylindrical portion That the first proceeding exercises a notable influence (at low velocities,) is generally admitted; that it virtually lowers the specific gravity of the ball and therefore retards the flight is no less certain; and if the rudder-like action of the grooves is admitted, their presence also entails a farther loss of velocity.

I have not as yet found time to experiment with either hollow or grooved balls, but the results obtained with solid projectiles, seem to show that it is of small moment whether *their* centre of gravity be situated a little before the middle of the longer diameter, or a little behind it; thus, in the double cone of which mention has been made the centre of gravity was forward of the middle of the axis, but in spite of this, it was found to require almost (if not quite) as many revolutions per second as ball No. 4. Balls were also constructed like Fig. 8 and fired with an initial velocity of 1682 feet when with a rate of 477 revolutions per second; they struck the target sidewise at 165 feet: a velocity of 1060 feet was then tried, when 268 revolutions proved wholly insufficient to project them point-foremost a distance of 12 feet.

8.

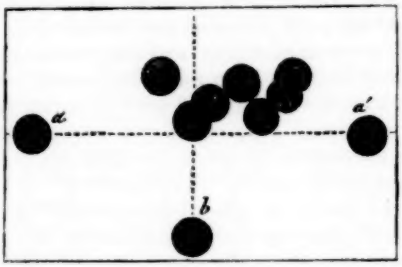
It may not be amiss in closing this article to offer a few remarks on the accuracy of the *American* rifle as compared with those now used in Europe.

In England it is admitted that the best practice has been obtained by Mr. Whitworth of Manchester with his hexagonal projectile 3 diameters in length, in a covered gallery 500 yards

* This Journal, vol. xxix, p. 197.

long. Mr. Whitworth states, or is reported to state, that he succeeds in projecting ball after ball into a circle but little more than two inches in diameter, and that he will not rest satisfied till he has fired a ball from one of his rifles down the barrel of another, placed at that distance! This excessive hopefulness of Mr. Whitworth might cause some to hesitate at the story of the two inch circle; assuming it however to be true, it may possibly be shown that the American rifle under like circumstances will do as well or better.

9.



One-half of full size.

The anexed target was made by Lewis with a rifle similar to No. 1, (provided only with globe and bead sights), at a distance of 220 yards, in the presence of over a hundred persons at the yearly trial of skill held at Waltham, Mass.—of course in the open air. The average distance of the shots from the centre is 1·038 inches. Any marksman upon inspecting it will at once see that shots *a* and *a'* were carried to the right and left merely by the wind, and that in a properly constructed gallery all the shots except *b*, would have been included in a circle one inch in diameter. It will also be acknowledged that in shots fired in a covered gallery the deviation with a good rifle is proportional to the distance traversed, though in the open air it is always somewhat greater owing to the wind producing more effect proportionally on the flight of the ball as its velocity becomes lowered.* Therefore as 220 yards : 500 yards :: 1 in. : 2·27, or nine out of the ten shots, at 500 yards, would have been in a circle 2·27 inches in diameter —practice as good as reported by Whitworth.

But what was the nature of the manipulation in each case? In Mr. Whitworth's by the help of "certain appliances with reference to the recoil, guarantees were obtained that each shot should be taken under similar circumstances. The gun was fitted accurately into a frame resting upon a perfectly level plane, and the recoil was compelled to take place in a line precisely

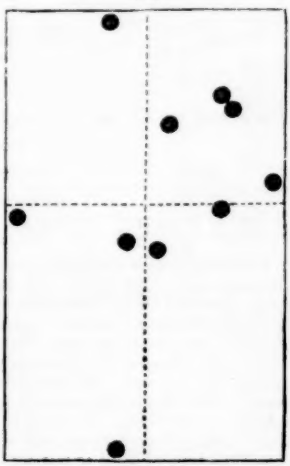
* The anterior probability of this statement may perhaps be rendered stronger by a comparison of the actual with this theoretic deviation. Rifle No. 1 when fired in the open air by only moderately skilled marksmen, gave—

Distance.	Theoretical Deviation.		Actual Deviation.
110 yd.	....	....	0·7 in.
220 "	1·4	+0·2	1·6 "
500 "	3·69	+3·02	6·71 "

parallel to its axis and could be calculated to a nicety. Again, when the object was to prevent any recoil, there was no difficulty in doing so."* All of the above mentioned precautions being no doubt essential to counteract the evil effects of the heavy recoil and of the *twisting* of the piece. With the American rifle no such artificial bolstering was employed, the barrel was rested on a common shooting bench and the stock held honestly to the shoulder of the marksman, the friction and recoil being so insignificant as not to require other contrivance.

But the Whitworth-gun was tried in the open air at Hythe, April, 1857, in competition with the Enfield rifle, to which it appears to have been greatly superior. We are not informed whether "the firing machine" was transported and used on this occasion; be that as it may, the statement made as to the results obtained is, "that when both had a range of 500 yards the superiority of the Whitworth was in the proportion of three to one." As the absolute deviation of the Enfield rifle at that distance is 28 inches, the deviation of the Whitworth rifle must have been about 9·3 inches, or the shots averaged that distance from the centre of the target. Chapman states that the absolute deviation of the American rifle at 550 yards is 11 inches; his work was published in 1848 since which time very considerable improvement has been effected by our best makers. Knowing this to be the case, I instituted some experiments at 500 yards with rifle No. 1: after it had been sighted for that distance ten shots were fired by a friend, who was but a moderate marksman; a reduced cut of the target is given. The distance of each shot from the centre was measured, the sum of the distances was 67·1 inches: the absolute deviation therefore in this experiment was $6\frac{7}{10}\frac{1}{0}$ inches or $2\frac{1}{2}$ inches less than that of the Whitworth rifle.

10.



One-tenth of full size.

This may serve to show how greatly the American rifle, as made by our best gunsmiths, must, and in fact does surpass in accuracy of fire the various rifles used at the present day in Europe, which are confessedly in this respect inferior to the Whitworth. By reference to the tables below it will be seen that the performance of Rifle No. 1 in the above target made at 500 yards, was superior to that of the Swiss rifle at 200 yards.

* "The Rifle," by Hans Busk, M. A., page 94.

No less demonstrable is it, that the American rifle-*pistol*, with a barrel only 12 inches in length, but constructed on the same principle as the larger arm, surpasses in the accuracy of its fire, up to a range of 500 yards, most of the rifles now used in Europe.* This may seem incredible; it is highly *significant* as pointing out, that the principles on which rifled-guns should be constructed, have been better apprehended by our countrymen, than thus far at least, by the ordnance boards of European governments.

Absolute Deviation.

	200 yd.	500 yd.
Enfield,	13·3	28·0 in.
Rifle à tige,	11·0	25·2 "
Swiss,	6·8	
American Government,	6·58	17·3 "

Absolute Deviation of 12 *in. Pistol according to Chapman. Weight of Ball* 100 *to* 115 *grains.*

110 yd.	220 yd.	330 yd.	440 yd.	550 yd.
in.	in.	in.	in.	in.
1·5 to 2	2 to 4	6 to 8	10 to 12	14 to 16

Diameters of Circles containing the best half of the shots.†

	164 yd	328 yd.	492 yd.
Swiss Federal,	5·4 in.	11·40 in.	23
Swiss Chasseur,	8·4 "	22·40 "	40
French Rifle à tige,	12·9 "	26·4 "	40
Austrian Rifle,	26·0 "	40 "	90!
Sardinian Rifle,	22·8 "	38 "	83

Diameters of Circles containing a certain proportion of the shots from the American 12*-inch Pistol.*

220 yd.	275 yd.	440 yd.	500 yd.
5·04 in.	9 inches.	30 in.	18 in.
8 out of 10 shots.‡	8 out of 10.§	All the shots.§	4 out of 10.‖

* Although Gen. Jacob has printed no particulars relative to his rifle practice at distances under 1000 yards, still that it cannot have been remarkably accurate, is patent from the table of the time of the flight of his projectiles given on page 35 of his "Rifle Practice," which is copied without comment by Lieut. Busk. page 165 of his work, and again by Lieut. Wilcox, page 210. By reference to either of these books it will be seen that the time of flight for 100 yards is 325 sec., for 200 yards exactly twice as much, for 300 yards just three times as much, and so on up to 600 yards; which is equivalent to saying that the velocity remained undiminished during a flight of 500 yards! A manifest absurdity, which can only be explained by the surmise that the path of the balls was irregular, and therefore allowed no determination of the time of flight to be made with accuracy. I was first unable to comprehend the meaning of this table, until in experimenting with a ball whose flight was known to be irregular, similar results were obtained. Gen. Jacob himself remarks that the table is a little curious.

† According to Lieut. Wilcox.
‡ Chapman, plate VI.
§ Trials by Mr. Lewis.
‖ An experiment of my own.

Strange indeed is it, that Lieut. Wilcox in his recent treatise on "Rifles and Rifle Practice," while describing each minute variation in the faulty construction of the European arm, should wholly ignore the existence of this most remarkable product of the experimental skill and mechanical ingenuity of his countrymen.

Inasmuch therefore as our own rifle for years has stood without a rival, how happens it that for the use of our army we have been induced to import an inferior arm from France? Were it not better policy to furnish our soldier with the weapon which has became so famous in the hands of our hunters? If greater accuracy or power be required, might it not be well to institute a minute investigation into the causes to which our home-product owes its success, rather than to spend time and incur expense in the study of the inferior rifles of Europe, which although owing their existence to the labors of boards of ordnance, composed of highly educated men, still have never approached in perfection the weapon devised by the *experimental skill* of our American backwoodsmen.

Below are a few of the initial velocities obtained in these experiments. Instead of the average merely, I have given each determination by itself. The powder employed was of very moderate strength, such indeed as is for the most part used in these rifles by our best marksmen:

**Rifle No. 2.*

Weight of ball 105·01 grains.

Charge.	Initial Vel.
50 grains about 2·15 inches of the bore,	1785
	1799
	1739
	1774
62 grains, about 2·66 in. the bore.	1890
	1925
	1925
	1928
	1917
75 grains, about 3·22 in. to the bore,	2082
	2086
	2084

Rifle No. 3.

Weight of ball 163·04 grains.

Charge.	Initial Vel.
69·4 grs. about 2·3 in. to the bore,	1759
	1782
	1816
	1785

Rifle No. 4.

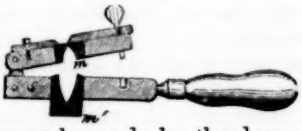
Weight of ball 113·35.

31·5 grs. about 135 in. to the bore,	1141
	1136
	1177
	1057
	1136
	1129

Fig. 11 is a section of an universal bullet mould which is per-

haps new. These experiments were greatly facilitated by its use. The portions *m* and *m'* are removable at pleasure. To construct a mould for a new ball, it is merely necessary to make the parts *m* and *m'*, all other labor being spared: an important consideration, where new forms of balls must be made by the dozen.

11.



Troy, February, 1860.

ART. IV.—*On the Conservation of Force;* by Prof. JOSEPH HENRY, Smithsonian Institution.*

[THE following remarks upon the conservation of force, particularly in relation to organic matter, by Professor Henry, Secretary of the Smithsonian Institution, will be interesting to those who have given attention to articles on the same subject, which have already appeared in this Journal.

They are extracted from the Agricultural Report of the Patent Office for 1857.]

Organic Molecules.—"The groups of atoms which we have thus far been considering, are principally those which have been formed under the influence of what is called the chemical force, and result from the ordinary attraction of the atoms. These are comparatively simple groups; but there is another class of groups of atoms of a much more complex character, and which are formed of new combinations of the ordinary atoms under the influence or, we may say, direction of that mysterious principle called the *vital force.* We are able to construct a crystal of alum from its elements by combining sulphur, oxygen, hydrogen, potassium, and aluminum; but the chemist has not yet been able to make an atom of sugar from the elements of which it is composed. He can readily decompose it into its constituents, but it is impossible so to arrange the atoms artificially, as in the ordinary cases of chemical manipulation, to produce a substance in any respect similar to sugar. When the attempt is made, the atoms arrange themselves spontaneously into a greater number of simpler and smaller groups or molecules than is found in sugar, which is composed of molecules of high order, each containing no less than 34 atoms of carbon, oxygen, and hydrogen.

The organic molecules, or atoms, as they are called, are built up under the influence of the vital principle of inferior groups of simple elements. These organic molecules are first produced in the leaves of the plant under the influence of light, and subsequently go through various changes in connection with the vital process. After they are once formed in this way, they may be

* Communicated by Prof. Henry.

combined and recombined by different processes in the laboratory, and a great variety of new compounds artificially produced from them.

But what is this vital principle, which thus transcends the sagacity of the chemist and produces groups of atoms of a complexity far exceeding his present skill? It is generally known under the name of the vital *force;* but since the compounds which are produced under its influence are subject to the same laws, though differing in complexity, as those produced by the ordinary chemical forces; and since in passing from an unstable to a more stable condition in the form of smaller groups, they exhibit, as will be rendered highly probable hereafter, an energy just equivalent to the power exerted by the sunbeam, under whose influence they are produced, it is more rational to suppose that they are the result of the ordinary chemical forces acting under the *direction* of what we prefer to call the vital *principle.* This is certainly not a *force,* in the ordinary acceptation of the term, or in that in which we confine this expression to the attractions and repulsions with which material atoms appear to be primarily endowed. It does not act in accordance with the restricted and uniform laws which govern the forces of inert matter, but with forethought, making provision far in advance of the present condition for the future development of organs of sight, of hearing, of reproduction, and of all the varied parts which constitute the ingenious machinery of a living being. Matter without the vital influence may be compared in its condition to steam which, undirected, is suffered to expend its power in producing mechanical effects on the air and other adjacent bodies, marked with no special indications of design; while matter under its influence may be likened to steam under the directing superintendence of an engineer, which is made to construct complex machinery and to perform other work indicative of a directing intelligence. *Vitality,* thus viewed, gives startling evidence of the immediate presence of a direct, divine and spiritual essence, operating with the ordinary forces of Nature, but being in itself entirely distinct from them.

This view of the subject is absolutely necessary in carrying out the mechanical theory of the equivalency of heat and the correlation of the ordinary physical forces. Among the latter, vitality has no place, and knows no subjection to the laws by which they are governed.

All the constituents of organic bodies are formed of organic molecules, and, as we have said, these are of great complexity, and are readily disturbed and resolved into a greater number of lesser groups. Thus, the constitution of cane sugar is represented by $C_{12}H_{11}O_{11}$, making in all 34 atoms. Organic bodies are, therefore, in what may be called a state of power, or of tottering

equilibrium, like a stone poised on a pillar, which the slightest jar will overturn; they are ready to rush into closer union with the least disturbing force. In this simple fact is the explanation of the whole phenomena of fermentation, and of the effect produced by yeast and other bodies, which being themselves in a state of change, overturn the unstable equilibrium of the organic molecules, and resolve them into other and more stable compounds. Fermentation, then, consists simply generally in the running down from one stage to another of organic molecules, changing their constitution, and at last arriving at a neutral state. There is, however, one fact in connection with the running down of the organic molecules which deserves particular attention, namely, that it must always be accompanied with the exhibition of power or energy, with a disturbance of the ethereal equilibrium in the form of heat, sometimes even of light, or perhaps of the chemical force, or of that of the nervous energy, in whatever form of motion the lattter may consist. It is a general truth of the highest importance in the study of the phenomena of Nature, that whenever two atoms enter into more intimate union, heat, or some form of motive power, is always generated. It may, however, be again immediately expended in effecting a change in the surrounding matter, or it may be exhibited in the form of one of the radiant emenations.

Balance of Nature.—The term balance of organic nature was first applied, we think, by Dumas, to express the relations between matter forming animals and vegetables, and the same matter in an inert condition. We shall apply the term "balance of nature," in a more extended sense, and include within it the balance of power, as well as the transformations of matter. The amount of matter in the visible universe is supposed to remain the same, though it is subject to various transformations, and appears under various forms—now built up into organic molecules, and now again resolved into the simple inorganic compounds. The carbon and other materials absorbed from the air by the plant is given back to the atmosphere by the decaying organisms, and thus what may be called a constant balance is preserved. But this balance, if we may so call it, does not alone pertain to the matter, but also to the energy which is employed in producing these changes. It may disappear for a while, or may be locked up in the plant or the animal, but is again destined to appear in another form, and to exert its effects, perhaps in distant parts of celestial space.

To give precision to our thoughts on this subject, let us suppose that all the vegetable and animal matter which now forms a thin pellicle at the surface of the earth were removed—that nothing remained but the germs of future organisms buried in the soil and ready to be developed when the proper influences

were brought to bear upon them. Let us further suppose the sun to cease giving emanations of any kind into space. The radiation from the earth, uncompensated by impulses from the sun, would soon reduce the temperature of every part of the surface to at least 60° below zero; all the matter and liquid substances capable of being frozen would be reduced to a solid state; the air would cease to move, and universal stillness and silence would prevail.

Let us now suppose that the sun were to give forth rays of heat alone; these would radiate in every direction from the celestial orb, and an exceedingly small portion of them, in comparison with the whole, would impinge against the surface of our distant planet, would melt the ice first on the equator, then on the more northern and southern parts of the globe, and, finally, their genial influence would be felt at the poles. The air would be unequally rarefied in the different zones, the winds would again be called forth, vapor would rise from the ocean, clouds would be formed, rain would descend, and storms and tempests would resume their sway.

If the sun should again intermit its radiation, all these motions would gradually diminish, and after a time entirely cease; the heat given to the earth would, in part, be retained for awhile, but in time would be expended; the water would slowly give out its latent caloric and be again converted into ice. Something of this kind takes place in the northern and southern parts of the earth during the different periods of summer and winter. Since the mean temperature of the earth does not vary from year to year, it follows that all the excess of heat of summer received from the sun is given off in winter and hence the impulses from this luminary which constitute all the energy, producing the changes on the surface of the earth, merely lingering for awhile, are again sent forth into celestial space, changed, it may be, in form, but not in the amount of their power. The solar vibrations have lost none of their energy, for the water has returned to the state of ice, and the surface of the earth is again in the same condition in which it was before it received the solar impulse. The energy of the solar vibrations communicated to the ice overcomes its cohesion, converting it into the liquid state, and the ice again becoming solid gives out the same amount of heat in a less energetic form. Even the motive power of the wind is expended by the friction of its particles in producing an amount of heat equivalent to that which gave rise to its motion, and this also is radiated into celestial space.

But the most interesting part of our inquiry relates to the effects which the radiation alone of heat from the sun would have on the vegetable germs buried in the soil. If these germs were enclosed in sacs filled with starch and other organic ingre-

dients, stored away for the future use of the young plant, as in the case of the tuber of the potato, or the fleshy part of the bean, as soon as the sun penetrated beneath the surface in sufficient degree to give mobility to the complex organic molecules of which these materials consist, the proper degree of moisture also supposed to be present, germination would commence. The young plant would begin to be developed, would strike a rootlet downward into the earth, and elevate a stem towards the surface furnished with incipient leaves. The growth would continue until all the organic matter in the tuber or sac was exhausted; the further development of the plant would then cease, and in a short time decay would commence.

But let us dwell a few minutes longer on the condition of the plant and the tuber before the downward action becomes the subject of consideration. If we examine the condition of the potato which was buried in the earth, we shall find remaining of it nothing but the skin, which will probably contain a portion of water. What has become of the starch and other matter which originally filled this large sac? If we examine the soil which surrounded the potato, we do not find that the starch has been absorbed by it; and the answer which will, therefore, naturally be suggested is, that it has been transformed into the material of the new plant, and it was for this purpose originally stored away. But this, though in part correct, is not the whole truth; for if we weigh a potato prior to germination, and weigh the young plant afterwards, we shall find that the amount of organic matter contained in the latter is but a fraction of that which was originally contained in the former. We can account in this way for the disappearance of a *part* of the contents of the sac, which has evidently formed the pabulum of the young plant. But here we may stop to ask another question: By what power was the young plant built up of the molecules of starch? The answer would probably be, by the exertion of the vital force; but we have endeavered to show that vitality is a *directing principle*, and not a mechanical power, the expenditure of which does work. The conclusion to which we would arrive will probably now be anticipated. The portion of the organic molecules of the starch, &c., of the tuber, as yet unaccounted for, has run down into inorganic matter, or has entered again into combination with the oxygen of the air, and in this running down, and union with the oxygen, has evolved the power necessary to the organization of the new plant.

The oxygen of the atmosphere penetrates into the interior of the potatoe, to enter into combination with the gluten and starch;—or, in other words, to burn it by a slow combustion; and the carbonic acid and water produced find their way, in turn, back to the atmosphere. We see from this view that the

starch and nitrogenous materials, in which the germs of plants are imbedded, have two functions to fulfill—the one to supply the pabulum of the new plant, and the other to furnish the power by which the transformation is effected, the latter being as essential as the former. In the erection of a house, the application of mechanical power is required as much as a supply of ponderable materials.

To return to our first supposition. We have said (and the assertion is in accordance with accurate observation) that the plant would cease to increase in weight under the mere influence of heat, however long continued, after the tuber was exhausted. Some slight changes might, indeed, take place; a small portion of pabulum might be absorbed from the earth; or one part of the plant might commence to decay, and thus furnish nourishment to the remaining parts; but changes of this kind would be minute, and the plant, under the influence of heat alone, would, in a short time, cease to exist.

Let us next suppose the sun to commence emitting rays of *light*, in addition to those of heat. These, impinging against the earth, would probably produce some effects of a physical character; but what these effects would be we are unable, at the present time, fully to say. We infer, however, that the light, not immediately reflected into space, would be annihilated; but this could not take place without communicating motion to other matter. It would probably be transformed into waves of heat of feeble intensity.

Let us now suppose, in addition to heat and light, the chemical rays to be sent forth from the sun. These would also produce various physical changes, the most remarkable of which would be in regard to the plant.

The carbonic acid of the atmosphere, in contact with the expanding surface of the young leaves, would be absorbed by the water in their pores, and in this condition would be decomposed by the vibrating impulses which constitute the chemical emanation. The atoms of carbon and oxygen, of which the carbonic acid is composed, would be forcibly separated; the atoms of oxygen would be liberated in the form of gas, and the carbon be absorbed to build up, under the directing influence of vitality, the woody structure of the plant. In this condition the pabulum of the plant is principally furnished by the carbonic acid of the air, while the impulses of the chemical ray furnish the primary power by which the decomposition and the other changes are effected. This is the general form of the process, leaving out of view minute changes, actions and reactions, which must take place in the course of organization.

In the decomposition of the carbonic acid by the chemical ray, a definite amount of power is expended, and this remains, as it

were, locked up in the plant so long as it continues to grow; but when it has reached its term of months or years, and some condition has been introduced which interferes with the balance of forces, then a reverse process commences, the plant begins to decay, the complex organic molecules begin to run down into simpler groups, and then again into carbonic acid and water. The materials of the plant fall back into the same combinations from which they were originally drawn, and the solid carbon is returned in the form of a gas to the atmosphere, whence it was taken. Now, the power which is given out in the whole descent, is, according to the dynamic theory, just equivalent to the power expended by the impulse from the sun in elevating the atoms to the unstable condition of the organic molecules. If this power is given out in the form of vibrations of the ethereal medium constituting heat, it will not be appreciable in the ordinary decay, say of a tree, extending, as it may, through several years; but if the process be rapid, as in the case of combustion of wood, then the same amount of power will be given out in the energetic form of heat of high intensity. This heat will again radiate from the earth: and in this case, as in that we have previously considered, the impulse from the sun merely lingers for a while upon the earth, and is then given back to celestial space, changed in form, but undiminished in quantity. It may continue its radiating course through stellar space, until it meets planets of other systems; but to attempt to trace it further would be to transcend the limits of inductive reason, and to enter those of unbridled fancy.

In the process we have described, the carbon, hydrogen, and other substances which are absorbed from the atmosphere, are returned to this great reservoir to be used again, and, it may be, to undergo the same changes many times in succession. The earthy materials are again returned to the earth, and all the conditions, as far as the individual plant which we are considering is concerned, are the same as they were at the beginning. The absorption of power in the decomposition of the carbonic acid gas, and its evolution again when the recomposition is produced of the same atoms, is precisely analogous to that which takes place in forcibly separating the poles of two magnets, retaining them apart for a certain time, and suffering them to return by their attractive force to their former union. The energy developed in the approach of the magnets towards each other is just equal to the force expended in their separation.

By extending this reasoning to the vast beds of coal which are stored away in the earth, we are brought irresistibly to the conclusion that the power which is evolved in the combustion of this material, now so valuable an agent in the processes of manufacture and locomotion, is merely the equivalent of the force which was expended in decomposing the carbonic acid which

furnished the carbon of the primeval forests of the globe; and that the power thus stored away millions of years before the existence of man, like other preordinations of Divine Intelligence, is now employed in adding to the comforts and advancing the physical and intellectual well-being of our race.

In the germination of the plant a part of the organized molecules runs down into carbonic acid to furnish power for the new arrangement of the other portion. In this process no extraneous force is required; the seed contains within itself the power and the material for the growth of the new plant up to a certain stage of its development. Germination can, therefore, be carried on in the dark, and, indeed, the chemical ray which accompanies light retards rather than accelerates the process. Its office is to separate the atoms of carbon from those of oxygen in the decomposition of the carbonic acid, while that of the power within the plant results from the combination of these same elements. The forces are therefore antagonistic, and hence germination is more rapid when light is excluded; an inference borne out by actual experiment.

Animal Organism.—Besides plants, there is another great class of organized beings, viz: animals; and as we commenced with the consideration of the seed in the first case, let us begin in this with the egg. This, as is well known, consists of a sack or shell containing a mass of organized molecules formed of the same elements of which the plant is composed, viz: carbon, hydrogen, oxygen, and nitrogen, with a minute portion of sulphur and other substances. Without attempting to describe the various transformations which take place among these organized molecules, a task which far transcends our knowledge or even that of the science of the day, we shall merely consider the general changes which occur of a physical character.

As in the case of the seed of the plant, we presume that the germ of the future animal pre-exists in the egg, and that by subjecting the mass to a degree of temperature sufficient perhaps to give greater mobility to the molecules, a process similar in its general effect to that of the germination of the seed commences. Oxygen is absorbed through some of the minute holes in the shell, and carbonic acid constantly exhaled from others. A portion then of the organic molecules begins to run down, and is converted into carbonic acid, and, possibly, water. During this process power is evolved within the shell—we cannot say, in the present state of science, under what particular form; but we are irresistibly constrained to believe that it is expended under the direction, again, of the vital principle, in rearranging the organic molecules, in building up the complex machinery of the future animal, or developing a still higher organization, connected with which are the mysterious manifestations of thought and volition.

In this case, as in that of the potato, the young animal, as it escapes from the shell, weighs less than the material of the egg previous to the process of incubation. The lost material in this case, as in the other, has run down into an inorganic condition by combining with oxygen, and in its descent has developed the power to effect the transformation we have just described.

We have seen, in the case of the young plant, that after it escapes from the seed, and expands its leaves to the air, it receives the means of its future growth principally from the carbon derived from the decomposition of the carbonic acid of the atmosphere, and its power to effect all its changes from the direct vibratory impulses of the sun. The young animal, however, is in an entirely different condition; exposure to the light of the sun is not necessary to its growth or existence; the chemical ray, by impinging on the surface of its body, does not decompose the carbonic acid which may surround it, the conditions necessaary for this decomposition not being present. It has no means by itself to elaborate organic molecules, and is indebted for these entirely to its food. It is necessary, therefore, that it should be supplied with food consisting of organized materials, that is, of complex molecules in a state of unstable equilibrium, or of power. These molecules have two offices to perform: one portion of them, by their transformations, is expended in building up the body of the animal, and the other in furnishing the power required to produce these transformations, and, also, in furnishing the energy constantly expended in the breathing, the pulsations, and the various other mechanical motions of the living animal. We may infer from this that the animal, in proportion to its weight before it has acquired its growth, will require more food than the adult, unless all its voluntary motions be prevented; and secondly, that more food will be required for sustaining and renewing the body when the animal is suffered to expend its muscular energy in labor or other active exercise.

The power of the living animal is immediately derived from the running down of the complex organized molecules, of which the body is formed, into their ultimate combination with oxygen, in the form of carbon, water and ammonia. Hence, oxygen is constantly drawn into the lungs, and carbon is constantly evolved. In the adult animal, when a dynamic equilibrium has been attained, the nourishment which is absorbed into the system is entirely expended in producing the power to carry on the various functions of life, and to supply the energy necessary to perform all the acts pertaining to a living, sentient, and, it may be, thinking being. In this case, as in that of the plant, the power may be traced back to the original impulse from the sun, which is retained through a second stage, and finally given back again to celestial space, whence it emanated. All animals are constantly radiating heat, though in different degrees, the amount

in all cases being in proportion to the oxygen inhaled and the carbon exhaled. The animal is a curiously contrived arrangement for burning carbon and hydrogen, and the evolution and application of power. In this respect it is precisely analogous to the locomotive, the carbon burnt in the food and in the wood performing the same office in each. The fact has long been established, that power cannot be generated by any combination of machinery. A machine is an instrument for the application of power, and not for its creation. The animal body is a structure of this character. It is admirably contrived, when we consider all the offices it has to perform, for the purpose to which it is applied, but it can do nothing without power, and that, as in the case of the locomotive, must be supplied from without. Nay, more, a comparison has been made between the work which can be done by burning a given amount of carbon in the machine, man, and an equal amount in the machine, locomotive. The result derived from an analysis of the food in one case, and the weight of the fuel in the other, and these compared with the quantity of water raised by each to a known elevation, gives the relative working value of the two machines. From this comparison, made from experiments on soldiers in Germany and France, it is found that the human machine, in consuming the same amount of carbon, does four and a half times the amount of work of the best Cornish engine. The body has been called "the house we live in," but it may be more truly denominated the machine we employ, which, furnished with power, and all the appliances for its use, enables us to execute the intentions of our intelligence, to gratify our moral natures, and to commune with our fellow beings.

This view of the nature of the body is the furthest removed possible from materialism; it requires a separate thinking principle. To illustrate this, let us suppose a locomotive engine, equipped with steam, water, fuel—in short, with the potential energy necessary to the exhibition of immense mechanical power; the whole remains in a state of dynamic equilibrium, without motion or signs of life, or intelligence. Let the engineer now open a valve which is so poised as to move with the slightest touch, and almost with volition, to let on the power to the piston; the machine now awakes, as it were, into life. It rushes forward with tremendous power, it stops instantly, it returns again, it may be, at the command of the master of the train; in short, it exhibits signs of life and intelligence. Its power is now controlled by mind—it has, as it were, a soul within it. The engine may be considered as an appendage or a further development of the body of the engineer, in which the boiler and the furnace are an additional capacious stomach for the evolution of power; and the wheels, the cranks and levers, the bones, the sinews, and the muscles, by which this power is applied.

ART. V.—*On a mode of employing Instantaneous Photography as a means for the Accurate Determination of the Path and Velocity of a Shooting Star, with a view to the Determination of its Orbit;* by JONATHAN H. LANE.

A tolerably accurate knowledge of the orbits of those meteors, or shooting stars, which may enter our atmosphere, would be of very high value in the settlement of certain questions as to their origin. Hitherto this knowledge has appeared unattainable by reason of the difficulty of effecting sufficiently precise observations of the meteor in the transient period of its visible flight, especially considering how this difficulty is aggravated on account of retardation of its motion by the resistance of the atmosphere. Recently, however, a method has occurred to me of applying instantaneous photography, so as to show accurately, not only the track of the meteor, but the division of its track into many equal and known fractions of time. If this can be successfully accomplished, we should have the data for ascertaining the velocity of the meteor at each point of the recorded part of its track, and the rate or law of variation of the velocity, and thence, with probably a good degree of accuracy, the velocity it had beyond the limits of the atmosphere, and the like remark may be made concerning the direction of the motion, should that be found subject to change.

The basis of the proposed process, as already intimated, is the extraordinary advances that have been made within a few years in the preparation of the sensitized surfaces of photographic plates, whereby artists are enabled to produce good pictures by an exposure of a very small fraction of a second—so small as to afford a tolerable definition of objects in motion, such as sailing vessels. This holds out encouragement for a hope, at least, that a passing meteor would leave a visible trace on a plate so prepared, or, even if that degree of sensitiveness has not yet been reached, that it will be hereafter. I need therefore make no apology for placing the suggestion on record previous to direct experiment on this point.

In the first place, simple exposure in a camera, at a given station, would give the apparent track of a meteor as seen by the observer at that station, and a pair of such records made in two cameras at two stations, would give the track in absolute space. In the second place, if one of the two cameras were furnished with a mechanism by which equidistant points of time should be marked upon the trace made in that camera, these points could be referred to the real path in space, and if both cameras were in like manner furnished, the two records would, to that extent be a check upon each other, and serve to reduce the limits of probable error. The

device for marking time is an application of the revolving glass prism, very similar to that described in my paper on a visual method of comparing time between distant stations, published in the January number of this Journal.* Immediately in front of the object glass of the camera, a glass prism of small angle and sufficient area to cover the entire aperture, is made to rotate at an accurately measured rate of say twenty-five revolutions per second. The prism may be replaced by an excentric lens, or the object glass itself may revolve on a slightly excentric axis. The consequence will be that the image of a fixed star in any part of the field of view will traverse the circumference of a circle every twenty-fifth of a second, and the image of a shooting star will combine this motion with its motion of translation. If the photographic surface retain a visible impression of the looped curve or the waved curve which will thus be produced, then, neglecting for the present the small effects of optical distortion, the line drawn midway between the two straight or regularly curved lines between which the looped or waved curve oscillates, will represent the apparent track of the meteor, and the points where it intersects the looped or waved curve, if they be translated along this middle line through a space equal to the optical displacement of the meteoric image, will show the apparent place occupied by the meteor at points of time separated by the equal intervals of one fiftieth of a second. If the period be made too brief, the impression left by the head of the meteor in one sweep of the looped or waved curve, might possibly be obliterated by the impression of the closely following parts of its train, while the head is traversing the subsequent sweeps of the curve. But there is no reason to anticipate from this cause any difficulty in obtaining a sufficiently short period to determine the law of variation of the velocity or direction.

In the above statement I have supposed only a single camera, but it will probably be impossible in this way to command a sufficient extent of the heavens. A system of many cameras may, however, be formed, so arranged that their several optic axes shall cross in a common point *in front* of the object glasses. The object glasses may thus be approximated as closely as we can desire, and the several revolving prisms, or excentric lenses, may have a common geared connection, and the backs of the cameras will be readily accessible for the renewal of plates. When the track of a meteor, by reason of its extent or situation, is obtained in parts from different cameras of such a system, it is geometrically impossible, on account of the spherical excess, that the exact interval of one fiftieth of a second between the times

* Vol. xxix, 43.

marked upon the meteor's track, should, in general, be preserved in the transition from one plate to another in all situations of the track, or in other words, that every two adjacent cameras in the system shall be capable of marking, in the manner described, the same common point of time upon the track of a meteor, but the exact difference in time can always be known.

In the execution of such a plan as this, two stations are to be selected at a suitable distance, and a system of cameras established at each, of such range that the two may cover in common a sufficient extent of the upper regions of the atmosphere to afford a fair chance for the occurrence of meteors. Each station will require at least an observer and a photographer. The photographer will renew the plates as often as their surfaces, either from time or exposure, become impaired, and will perform the manipulations required in fixing the impression when taken. The observer, after having made the necessary instrumental adjustments and determinations will be charged with the sole duty of watching for meteors in the region covered by his system of cameras, and at the appearance of a meteor will touch a spring so contrived as to cause the instant unveiling of all the cameras of the system, and on the extinction of the meteor will promptly replace the screen.

The expense and trouble of this process will certainly be great, but will not be disproportioned to the importance of the object in view. Only let us have a photographic surface that will give a visible trace of the meteor's path, in the face of exposure to the light of the sky during the time of the meteor's visible flight, and then success, as regards the attainments of an accurate record, will be nearly certain, and we should not hesitate at the expense and trouble.

Nor does it seem to me that our success would be much less certain in respect to the reliable determination of the direction and velocity which the meteor had before entering the atmosphere, and consequently of the orbit in which it had moved. A very simple calculation based upon the mechanical theory of heat, leads us to the conclusion that any body of a nature to become readily incandescent by heat, of such a thickness as half an inch, and not possessing a greater power of conducting heat through its mass than any we are acquainted with, must, on entering our atmosphere with planetary velocity, become self luminous by the time that velocity has been reduced by some such fractional part as one thousandth. The *vis viva* of a body moving with a velocity of twenty miles a second is equivalent to the heat that would raise the temperature of an equal weight of water about 224,000° Fah. With such a velocity, so many times exceeding that of sound, the masses of air lying in the

path of the body must be driven before it, and receive a velocity equal to that of the body, or at least to a large fractional part of it. The mass of air which the body must have encountered in losing the thousandth part of its velocity, will, therefore, be of the order of a thousandth part of that of the body. With the loss of a thousandth part of the velocity, the loss of the body's own *vis viva* will correspond to the quantity of heat that would raise the temperature of its weight of water 448° Fah. If after making allowance for the motion communicated to the displaced air—approximately one half—and for the quantity of generated heat which this air retains and carries off, we assume that a twentieth part of the above 448° enters into the body itself, and by reason of the rapidity of its production is collected in a superficial coating of a hundreth part of its mass, and give this a specific heat within that of water, we should find an elevation of temperature of 2240° or upwards. The inference we would draw from these considerations seems confirmed by what we know of the great length of the visible flight of meteors, and of the great elevation of the region of atmosphere in which it occurs.

If, therefore, upon suitable trials made upon the fixed stars, and upon shooting stars themselves, we shall find ourselves in possession of sufficient photographic power, there is no reason why an organized system of observations should not be instituted. If the fact in regard to the retardation of a meteor's motion be as the foregoing considerations lead us to anticipate, the discussion of a collection of such records as we should obtain, of a large number of meteors, will be likely to afford us complete assurance on the subject, by pointing out certain laws of the resistances at different altitudes. A moderate degree of accuracy in the absolute determination of the orbits, except, when they make a near approach to the parabola, will be sufficient to answer all the questions of interest that will be likely to arise upon which a knowledge of the orbits would have any bearing. Whether the November meteors, for instance, move through regions that would identify them with the Zodiacal light, according to the theory of the late Prof. Olmsted, is a question that would receive an absolute determination.

ART. VI.—*The True Figure of the Earth.*—Notice by Mädler in Prof. Heis' Wochenschrift für Astronomie, Meteorologie und Geographie, No. 51 and 52. Dec. 21 and 28, 1859.*

Essai d'une détermination de la véritable figure de la terro. Par T. F. DE SCHUBERT. St. Petersbourg, 1859.

THIS brief but very important paper treats of a question which has engaged mankind for ages, and treats it in such a way as to convince us that an essential step forward has been taken towards its final determination.

It is not intended to recapitulate the history of opinions and notions which antiquity, as well as the middle ages, had formed on this topic so generally interesting, as it does not belong to the plan of the work to be discussed.

Since the establishment of Newton's theory, that the earth is flattened at its poles, and the confirmation of this theoretical result by the first measurements of arcs in the eighteenth century, the chief inquiry has been directed to the magnitude of this compression, or, in other words, the difference between the semi-major and the semi-minor axes of the earth. The attempt was made to determine it by comparing arcs measured in different latitudes, the latitudes of their extremities being astronomically and their extent on the earth geodetically determined. In this manner was obtained, between 1735 and 1746, two arcs, the one in Peru, and the other in Lapland, which could be compared with each other, as well as that previously measured by Cassini in France. The result of the comparison was not satisfactory. Although they proved a comparison, the measures did not agree, and the source of this difference was too obscure to favor the supposition that any nearer approach had been made to the object of research.

In the course of the eighteenth century measures of arcs of the meridian were executed or attempted in France, Austria, Italy and Pennsylvania, at the Cape of Good Hope and other places. Their comparison made it apparent that the compression ($\frac{1}{1}\frac{1}{7}\frac{1}{6}$), as at first concluded, was too great, and that it must be reduced one half or even more. Yet the uncertainty remained very considerable.

The measurement of arcs was continued in the nineteenth century over greater extents in different parts of the earth and with greater care and accuracy, and the close agreement of these arcs, as it appears from the comparison of their parts, left but little, further to be desired. Perceptibly different values for the compression were nevertheless obtained, as when the most proba-

* Translated for the American Journal of Science by CHARLES A. SCHOTT, Assistant, and communicated by Prof. A. D. BACHE, Supt. U. S. Coast Survey.

ble value was determined from them all, there still remained from the separate measures of arcs, deviations which could not be ascribed to errors of observation.

Repeated investigation has shown that the old measurement of a degree by Maupertuis and Outhier was considerably in error. Rosenberg and Swanberg obtained quite different values. In like manner the measures executed by Boscovich, Le Maire, Liesganig, Beccaria and others in the eighteenth century could not bear severe criticism nor stand side by side with the better and approved arcs. A more accurate and reliable coefficient of compression still remains a desideratum.

We insert here for comparison the values of the compression, as they result, for the most part, from the same measures, but according to different methods of reduction, in the first four decades of this century.

	Semi-major axis in toises.	Semi-minor axis in toises.	Compression in toises.	Coefft. of compression.
Walbek, - -	3271742·8	3260989·3	10785·5	$\frac{1}{302\cdot78}$
Schmidt, - -	3271852·3	3260853·7	10998·6	$\frac{1}{297\cdot48}$
Bessel, (I.), -	3271953·9	3261072·9	10881·0	$\frac{1}{300\cdot70}$

As Bessel afterwards discovered an error in Delambre's French triangulation which could not be without influence on the combined result, he repeated his reduction with this correction and found—

Bessel, (II), -	3272077·1	3261139·3	10937·8	$\frac{1}{299\cdot15}$

Still, according to the last reduction, the probable error is three times as great as the difference of Bessel (I) and Bessel (II).

Since in this even the best measures of this sort do not yield sufficiently accordant results, it was attempted to find the compression in two other ways. The observations of the pendulum give, as a consequence of the earth's compression, different lengths of the seconds pendulum in different latitudes, and hence we can deduce, conversely, the compression from these observed lengths or, if we choose, from the number of beats which the same pendulum makes in the course of a day. Observations made with extreme care were used as the foundation of such experiments, yet still the result ($\frac{1}{288\cdot20}$) fluctuated between the limits, $\frac{1}{285}$ and $\frac{1}{291}$.

The moon's path, on which the compression was not without influence, was also resorted to, and gave $\frac{1}{305}$ for the compression, but on account of the smallness of the aggregate effect it is of inferior accuracy.*

We have found it necessary to present this general view in order to define the limit of the results arrived at by preceding

* This result depends, in a measure, on the law of density of the earth in passing from the surface to the centre.

efforts, and in order to place the merit of the writer in its proper light, especially for those not possessing a special knowledge of the subject in question.

The preceding investigations were all based upon the following assumptions:

(1.) The meridians of the earth are ellipses.*

(2.) The minor axis is also the axis of rotation.

(3.) All the meridians of the earth are equal.

The writer remarks that, in a rigorous sense, no one of these assumptions is proved and that we do not possess the means of proving the first two. We may however add, that these two assumptions, if not absolutely, must be very nearly true.†

In regard to the third assumption we are now prepared to submit it to investigation, and the previous failures compel us to question its applicability. Paucker and Borenius have already attempted to prove that it must be false, but neither has arrived at any definite result. The writer uses as a basis the following measurements of arcs:

(1.) The Russian (or more properly Russo-Scandinavian) executed in 1820–51 by Hansteen, Selander, Struve and Tenner from Fuglenäs (in latitude 70° 40′ 11″ ·3 N) to Staro-Nekrasofka (45° 20′ 02″ ·8 N) the longest arc yet measured.

(2.) The Indian arc, 1802–43, by Lambton and Everest, from Kaliana (29° 30′ 48″ ·9 N) to Punnæ (8° 09′ 32″ ·3 N).

(3.) The French arc, 1792–1806, by Méchain, Delambre, Biot and Arago, from Dunkirk (51° 02′ 08″ ·5 N) to Formentera (38° 39′ 56″ ·1 N).

(4.) The measure at the Cape of Good Hope, by Henderson and Maclear, from 34° 21′ 06″ ·3 S to 29° 44′ 17″ ·7 S.

(5.) The Peruvian arc, 1735–46, by Bouguer and la Condamine, from Tarqui (3° 04′ 32″ ·1 S) to Cotchesqui (0° 02′ 31″ ·4 N).

(6.) The Prussian arc, 1831–34, by Bessel and Bæyer, from Memel (55° 43′ 40″ ·4 N) to Trunz (54° 13′ 11″ ·5 N).

(7.) The British arc, by Roy and Mudge, from Clifton (53° 27′ 31″ ·1 N) to Dunnose (50° 37′ 07″ ·6 N).

(8.) The Pennsylvanian arc, 1764, by Mason and Dixon, from 39° 56′ 22″ ·5 N to 38° 27′ 37″ ·5 N.‡

The writer does not explain why he has not taken in other reliable measures, in particular the Hanoverian by Gauss and the Danish arc by Schumacher; but his principal aim being to prove merely the untenability of the assumption that all meridians are equal and similar, and pronouncing his own work as preliminary, no exception can be taken to this course. In a perfectly rigorous investigation by the method of least squares, where each

* In the recent account of the Ordnance Survey of Great Britain the figure of the meridian, in one of the hypotheses, is not restricted to this condition.

† The truth of the second assumption may be granted.

‡ The reason for taking in this arc will be apparent from what follows.

measure with its corresponding weight would enter, such omissions could of course not be permitted.

Reducing and comparing with each other, each of the eight arcs, he obtains twenty eight binary combinations, which he communicates in detail; but this can only be for the purpose of showing the impossibility of an agreement by the assumption hitherto made. For instance, the Russian arc compared with the English gives $\frac{1}{311.7}$, the French compared with the English gives $\frac{1}{317.4}$, and the deviations from the general mean are still greater in comparing the Prussian and Pennsylvanian arcs with all the others. It is thence inferred that the meridians of the earth are not equal to each other, and thence that the equator and the parallels are not circles, so that it is generally impossible to draw a great circle on the earth's surface anywhere.

Meridians different in form and length really indicate different polar compressions, a non-agreement of the results found, proving nothing against the accuracy of the measures.

Now since all meridians must converge at the poles they must all have in common one and the same diameter, viz., the smallest (the axis of rotation) which can be obtained from each of the large arcs referred to.

The three greatest measured arcs are—

the Russo-Scandinavian	=	25° 20′ 08″·5,
" Indian	=	21 21 16 ·6,
" French	=	12 22 12 ·4,

whilst all the others, including those not used by the writer, are less than 5° in extent.

By dividing each of these three arcs into two equal parts and comparing one part with the other and also each part with the whole, the writer obtains the following mean values:

	Semi-major axis (toises).	Semi-minor axis (toises).
From the Russian	3272610·3	3261428·7
" " Indian	3272650·9	3261547·4
" " French	3273448·2	3260364·7

In the first two, the differences are unimportant, but the last shows a greater deviation. Schubert remarks that this latter deviation can be got rid of by changing the latitude of Carcassonne, the selected point of division of the French arc, by 1″·96, which may not exceed the limits of uncertainty. He considers it safer, however, to obtain the semi-minor axis from the first two arcs alone, the greater arc (the Russian) having twice the weight of that of India. In this manner he deduces the semi-minor axis:

3261467·9 toises.

By means of this value the semi-major axis can be found for each measure of an arc. It results as follows:

For the meridian of Kaliana	in Long. 95° 20′	3272581·3
" " " Dorpat	" " 44° 23′ 10″	3272650·1
" " " Tarqui	" " 298° 44′	3272382·8

Three radii and their included angles suffice for the determination of the ellipse. The writer finds for the semi-major axis of the equatorial ellipse 3272671·5 toises
and its direction 58° 44′
for the semi-minor axis of the equatorial ellipse 3272303·2
and its direction 148° 44′

Compression of the equator..... = $\frac{1}{\quad}$;
Polar compression of the greatest meridian = $\frac{1}{\quad}$
" " " smallest " = $\frac{1}{\quad}$,

and the separate arcs calculated with these values have the following deviations:

	in toises.	in arc.
Peru,.....	+ 1·2	0″·077
Pennsylvania,.....	−105·68	6 ·687
England,.....	+ 11·68	0 ·736
France,.....	− 25·45	1 ·607
Cape of Good Hope,.....	− 6·98	0 ·442
Prussia,.....	+ 23·28	1 ·267
Russia,.....	− 20·47	1 ·289
India,.....	+ 25·54	1 ·619

In reference to the arcs showing greater deviations the writer remarks that the Pennsylvanian arc was measured with imperfect means (by the chain only, and without triangulation) and that he has taken it merely for trial.* The other seven deviations are so small that no one exceeds the ten thousandth part of the measured length, and the great accuracy of the Peruvian measure, which had been already shown in former discussions, appears here in its true excellence. Honor is justly due to the memory of the men who, at so early a period, accomplished such a work under the untold difficulties of ten years' separation from all civilized countries.

It has frequently been shown that the geodetic measures, in particular the longitudes, do not harmonize with astronomical observations, but no one has yet succeeded in discovering the cause of this disagreement. The ellipticity of the earth's equator, as discovered by the writer, will call forth new investigations,

* Dr. Maskelyne in his description of Mason and Dixon's base (Phil. Trans., London, 1768) says they employed *rods of fir* frequently compared with a standard brass measure at a fixed temperature. This line was revised in 1849 and 1850 by Lt. Col. Graham, U. S. Top. Eng. See Message of the Gov. of Md., &c., in relation to the intersection of the boundary lines of the States of Maryland, Pennsylvania and Delaware. Washington, 1850.

since it shows that each geodetic survey, and particularly the longitudinal differences, require correction. The writer shows from two examples how we must proceed and what we may expect.

Between Pulkowa and Dorpat the difference of longitude was found as follows:

Astronomical	3°	36′	16″·77	
Geodetic	3	36	23 ·07′	
Difference			+6″·94	= 57·9 toises.
The correction now found to the geodetic is—			−6″·01	= 50·1 toises.
and the remaining error			0″·93	= 7·8 toises.

Between Pulkowa and Warsaw is found—

Astronomical	9°	17′	48″·43	
Geodetic	9	18	01 ·24	
Difference			+12″·81	= 124·7 toises.
Correction to the geodetic			−18 ·85	= 183·4 toises.
remaining error			= − 6 ·04	= 58·7 toises.

The first error is thus completely got rid of and the second is reduced to nearly one half.

The writer concludes with the following remarks:

"This determination of the earth's figure is only an approximation, subject to many improvements, when we have more data, and when we use more rigorous methods of reduction. But it shows that we can obtain an agreement of the measures in which we have not heretofore succeeded. The determination of the general figure of the earth does not exclude local irregularities of its surface.

When this hypothesis of the earth's figure, after a scrutinizing investigation, finds general adoption, all geographical positions determined by geodetic means must have corrections applied, which will refer principally to the differences of longitude."

As much as I may agree with this view and as certainly as the importance of the object requires the application of rigorous methods, it is yet clear that the trouble will be sufficiently rewarded when further reliable data, particularly from the western hemisphere, shall be available to the computer. It may be remarked here that the meridian in the western hemisphere corresponding to the small equatorial axes passes through Newfoundland, and, in the eastern, through the Amoor country and Eastern Siberia. For these countries we have at present not even approximate determinations. The Pennsylvania measure of 1764 is worthless for accurate investigation and the superior Peruvian measure is at too great a distance. Measurements of

arc in the meridian of Irkutsk and extending through Mantchooria southwards, through the Chinese empire, as well as the Atlantic States of the American Union* will give us the smallest meridian of the earth with the same accuracy, corresponding to the determination of the greatest, which passes through Eastern Europe (Kostromo, Stawropol, Erzerûm). Still, much requires to be done. The continuation of the Russian Survey through Turkey, and if possible to Eastern Africa, would be most important; and Struve has already (in 1857) advocated it in the Paris Academy. The opposite part of this greatest meridian passes over the icy deserts of North Western America and the group of islands known as the Marquesas, and hence afford no possibility of its completion on that side.

After this article was printed, the writer received the account of the New British Survey and notice of the correction of a small error of computation. This induced him to reprint some of the sheets on which the numerical results are given as above.

It will hardly be necessary to add anything in praise of the author. To bring up a question of such importance is sufficiently praiseworthy in itself, but the writer has done more; he has opened the way to a final determination and has given it, at least in its general aspect. We shall rejoice if this interesting subject calls other powers into the field to labor further in this direction. Corrections like those which the writer has made for Dorpat and Warsaw will be required for many other places, and each comparison of this kind furnishes its share to the final determination. MÄDLER.

ART. VII.—*On the Transit Instrument as a substitute for the Zenith Telescope in determining Latitude, and on the Latitude of New Haven;* by Prof. C. S. LYMAN.

THE method of determining latitude by measuring micrometrically the difference of meridional zenith distances of stars on opposite sides of the zenith, known commonly as Talcott's method, strongly commends itself to observers, both by its beautiful simplicity, and the very great accuracy of its results. It is the method of late years exclusively used in the operations of the United States Coast Survey, and may properly be regarded as one of the many excellent incidental fruits of that great National work. A special instrument, however, the zenith telescope, has been required for making the necessary observations, and sup-

* It may be stated in connection with this subject that the geodetic surveys of two arcs, one in the New England States, the other on the Chesapeake Bay, are already completed and the astronomical part of the former is nearly so, promising an important result derived from the labors of the U. S. Coast Survey at no distant time.

posed to be indispensable for this purpose. It indeed admirably answers the end for which it was invented, and in respect to simplicity, convenience, and efficiency, leaves almost nothing to be desired. But being expensive, and of limited use, it is likely to be in the hands of but few observers. The suggestion, therefore, of additional instrumental facilities for using this method of latitude, so as to render it more widely available, may not be considered as without value.

At the recent meeting of the American Association, at Springfield, the writer, in a brief communication, pointed out the facility and efficiency with which a transit instrument may be used as a substitute for the zenith telescope, in such observations. The object of the present article is, in part, to exemplify further the same point, and at the same time to place on record the results of a more careful determination, astronomically, of the latitude of New Haven, than, so far as appears, has heretofore been attempted.

The essential parts of the zenith telescope, as an instrument for determining latitude by Talcott's method, are its level and micrometer. And the superiority of the method itself rests mainly on the fact that its results depend on these simple and efficient instrumental means, instead of on graduation. It is obvious, that a transit instrument with a declination micrometer and a suitable level attached to its finding circle, is, at the same time, essentially a zenith telescope, and is capable of performing the whole work of that instrument—as accurately, if of corresponding size and quality, and as conveniently, if furnished with a reversing apparatus. The advantages of such a use of the transit instrument, in a multitude of cases, scarcely need to be pointed out. Where both latitude and longitude are to be determined at the same station, as is usually the case, a single instrument will suffice for both, and thus half the ordinary equipment for the purpose may be dispensed with. The corresponding diminution of expense will be a consideration of weight where pecuniary means are limited, as will also the saving of transportation, in the case of boundary and other surveys in remote and uninhabited districts. The observer who has at command such an instrument, even of very moderate size, will have it in his power to fix his latitude with a degree of precision scarcely surpassed even in first class observatories.

The transit instrument with which the observations were made, the results of which are given in this article, has an object glass (by Fitz) of two and six-tenths inches aperture, and thirty-five and a half inches focal length; a filar-micrometer, so constructed as to be used with equal facility either in the plane of the meridian or perpendicular to it; and a twelve inch circle, (graduated on the new and excellent engine of Messrs. E. & G. W.

Blunt, of New York,) reading by two verniers to 10″. The level attached to the alidade of the circle has a run of one-thirteenth of an inch for 5″; and this being the smallest division of the scale, single seconds and fractions have to be estimated by the eye—a defect which will be further noticed in another connection.

The graduation of the circle is very accurate. The difference between the readings of the opposite verniers seldom amounts to to 15″, and from its law of variation, it is manifestly due, in the main at least, to a very slight excentricity in centering the circle on the graduating engine, not to imperfection in the engine itself.

The micrometer-screw has 75 threads to the inch, or one revolution of the head equal to 78″·74.

The optical performance of the instrument is very satisfactory. Transits of Polaris are readily taken at midday, and in favorable states of the atmosphere, at that hour, even by reflection in mercury. Transits of Aldebaran have been taken when the star was within 6° of the sun's center. The eye-piece commonly used is a diagonal one of four lenses, with prismatic reflector,—the magnifying power about 190.

The method of observing for latitude with the transit instrument is of course essentially the same as with the zenith telescope. A pair of stars having been selected, which culminate within a few minutes of each other, but on opposite sides of the zenith, and having their zenith distances nearly equal, the instrument is set for the star first to culminate, the level clamped, the bubble brought to zero by the slow-motion screw which turns the whole instrument, the star bisected with the micrometer wire, and the readings of the level and micrometer noted. The axis is then reversed, the bubble brought to zero as before, and the other star observed in the same manner.

The pairs for the observations now to be considered, were taken from the Catalogue of the British Association. The stars are to the sixth magnitude, and mostly within 25° of the zenith, the difference of zenith distances of the stars of a pair being usually less than 25′—averaging about 12′.

The individual results of 92 pairs are exhibited in the following table, which includes, with two exceptions, all the observations made, whether in favorable or unfavorable states of the atmosphere. In a few cases where the B. A. C. differs widely from other catalogues, the B. A. C. results are enclosed in brackets [ ] and are omitted in computing the mean. That the fault is in the B. A. C., is obvious from the accordance of the corresponding results from other catalogues with the general average.

The contents of the table are as follows:

Col. 1 and 2. The numbers from the British Association Catalogue designating the stars of a pair.

3. The dates of the observations.

4. The mean results for latitude from each pair, using the positions of the B. A. C.; the seconds of the individual results, where the same pair was observed on different evenings, being placed at the left hand side of the column, and connected with their mean by a brace.

5. The differences, Δ, between these mean results of pairs and the mean of the whole.

6. The results for latitude from the same observations, the position of one or both the stars of the pair being taken from some other catalogue than the B. A. C.; the arrangement as in column 4.

7. The names of these other catalogues; G. standing for Greenwich Twelve Year Catalogue; B. that of the British Association; R. the catalogue in Radcliffe Observations for 1856; and A. the English Nautical Almanac. A. G., for example, signifies that the position of the first star of the pair is from the Nautical Almanac, and that of the other from the Twelve Year Catalogue; etc.

8. The differences between the results in column 6 and the mean of the whole.

Number of each Star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
121	164	1858, Nov. 8	43″·4	43″·7	+1·3	42″·3	42″·6	B. G.	+0·2
"	"	" " 11	44 ·1			43 ·0			
155	219	" " 10		40·0	−2·4		39·8	G. B.	−2·6
245	334	" " 11		42·1	−0·3		42·0	B. G.	−0·4
267	314	" Dec. 16		45·3	+2·9		45·3	B. G.	+2·9
334	487	" Nov. 10	42·1	42·5	+0·1	41·7	42·2	G. G.	−0·2
"	"	" " 11	43·0			42·7			
1083	1123	1859, Jan. 20		45·0	+2·6		43·0	R. G.	+0·5
1175	1214	" " "		40·7	−1·7		42·1	B. R.	−0·3
1313	1367	" " "		43·5	+1·1		42.0	R. R.	−0·4
3523	3777	" Oct. 21		41.0	−1·4		40·2	A. A.	−2·2
3742	3767	" May 6		42·6	+0·2		42·4	B. G.	0·0
3767	3809	" " "		42·6	+0·2		42·4	G. B.	0·0
3856	3913	" " "		41·8	−0·6		42·2	G. R.	−0·2
3968	3995	" " "		40.8	−1·6		40·5	B. G.	−1·9
4112	4151	" " "		43·9	+1·5		43·0	G. G.	+0·5
4216	4240	" " "		42·7	+0·3				
4351	4371	" " "		42·8	+0·4				
4371	4406	" " "		42·9	+0·5		42·1	B. G.	−0·3
4789	4823	" June 30		42·8	+0·4		45·2	G. R.	+2·8
4980	5036	" " "		44·0	+1·6		43·9	G. R.	+1·5
5131	5181	" July 5		41·5	−0·9				
5245	5285	" June 30	40·5	40·3	−2·1	41·0	40·8	R. A.	−1·6
"	"	" July 5	40·1			40·6			
5285	5309	" June 30		40·9	−1·5		40·8	A. B.	−1·6
5336	5388	" " "		43·1	+0·7		44·2	B. R.	+1·8
5406	5490	" July 5		41·5	−0·9				
5417	5460	" " 6		40·7	−1·7				
5460	5523	" June 30		43·7	+1·3		42·8	B. G.	+0·4

Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
5449	5604	1859. June 30	43″·4	43″·7	+1·3	43″·6	43″·9	B. G.	+1·5
"	"	" July 5	44″·1			44 ·3			
5525	5560	" " 6		43·8	+1·4		42·6	R. R.	+0·2
5643	5703	" June 30	40·7						
"	"	" July 5	41·9	41·5	−0·9				
"	"	" " 6	41·9						
5703	5752	" June 30		[47·2]	+[4·8]		42·7	B. R.	+0·3
5747	5776	" July 5	40·8	40·2	−2·2	40·5	39·9	G. B.	−2·5
"	"	" " 6	39·5			39·2			
5776	5842	" " 5	41·4						
"	"	" " 6	42·4	41·9	−0·5				
5841	5887	" Aug. 16		40·5	−1·9				
5951	6021	" July 5		42·9	+0·5		42·4	G. A.	0·0
6033	6079	" Aug. 16	[56·1]			43·8			
"	"	" " 29	[57·2]	[56·7]	+[14·3]	44·9	44·4	R. R.	+2·0
"	"	" Sept. 1	[56·7]			44·4			
6150	6185	" Aug. 29		42·3	−0·1		41·9	G. B.	−0·5
6162	6193	" Sept. 1		43·1	+0·7				
6178	6246	" Aug. 16		43·1	+0·7		41·7	G. G.	−0·7
6185	6238	" " 29		41·5	−0·9				
6223	6289	" Sept. 1	40·7	40·8	−1·6	41·5	41·6	G. G.	−0·8
"	"	" " 9	40·9			41·7			
6289	6322	" Aug. 29	43 0	43·5	+1·1	43·2	43·7	G. G.	+1·3
"	"	" Sept. 7	44·1			44·3			
6404	6473	" Aug. 16	41·2	41·2	−1·2				
"	"	" Sept. 9	41·2						
6427	6470	" Aug. 29		43·8	+1·4				
6520	6556	" " 16	43·6			41·9			
"	"	" " 29	45·4	44·4	+2·0	43·7	42·7	R. B.	+0·3
"	"	" Sept. 1	44·3			42·6			
6583	6637	" Aug. 16	42·4	43·6	+1·2	42·3	43·5	G. B.	+1·1
"	"	" Sept. 9	44·8			44·7			
6623	6648	" " 1	41·0			41·9			
"	"	" " 26	43·2	41·7	−0·7	44·0	42·6	G. G.	+0·2
"	"	" Oct. 4	41·0			41·9			
6662	6744	" Sept. 7		43·2	+0·8		43·7	G. B.	+1·3
6674	6712	" Aug. 29	[51·5]	[50·8]	+[8·4]	43·7	43·0	G. R.	+0·6
"	"	" Sept. 9	[50·1]			42·3			
6690	6748	" Aug. 16	42·5	42·5	+0·1	42·0	42·0	G. B.	−0·4
"	"	" Sept. 26	42·5			42·0			
6709	6737	" " 1		41·6	−0·8				
6731	6765	" Oct. 4		44·5	+2·1				
6741	6784	" Aug. 29	[38·5]	[37·8]	−[4·6]	43·8	43·0	R. R.	+0·6
"	"	" Sept. 9	[37·0]			42·3			
6779	6849	" " 26		41·5	−0·9		42·2	G. B.	−0·2
6799	6851	" " 7	42·1	41·6	−0·6				
"	"	" Oct. 7	41·1						
6827	6867	" Aug. 29	43·2			43·4			
"	"	" Sept. 9	43·1			43·3			
"	"	" Oct. 3	43·4	43·1	+0·7	43·5	43·3	B. R.	+0 9
"	"	" " 4	42·8			42·9			
6858	6930	" Sept. 1	42·4	42·5	+0·1				
"	"	" Oct. 6	42·6						
6893	6936	" Sept. 7	42·7	42·7	+0·3	41·2	41·2	G. B.	−1·2
"	"	" " 26	42·6			41·2			
6926	6952	" Aug. 29	41·4	41·3	−1·1	41·6	41·5	G. B.	−0·9
"	"	" Oct. 4	41·2			41·4			
6932	6975	" " 3		42·7	+0·3		42·4	R. B.	0 0
6937	6965	" " 7		45·3	+2·9		43·2	G. G.	+0·8

Number of each star in B. A. C.		Date of observation.	Lat. by B. A. C.		Δ	Lat. by other catalogues.		Name of catalogue.	Δ
			41° 18′			41° 18′			
6965	6997	1859, Sept. 1	42″ 6			42″·2			
"	"	" " 9	44 ·0	43″·5	+1·1	43 ·6	43″·1	G. B.	+0·7
"	"	" Oct. 7	43 ·8			43 ·4			
6959	7029	" Sept. 7		45·4	+3·0				
6963	7022	" " 26		44·4	+2·0		44·6	B. G.	+2·2
6996	7041	" Oct. 3		40·8	−1·6				
7037	7107	" Aug. 29		40·9	−1·5		40·3	B. G.	−2·1
7041	7114	" Oct. 3		39·8	−2·6				
7067	7153	" Sept. 7		40·5	−1·9				
7088	7124	" Oct. 4	40·3			39·8			
"	"	" " 7	39·9	40·1	−2·3	39·4	39·6	G. B.	−2·8
7119	7174	" Sept. 9		40·7	−1·7				
7246	7281	" " 7	42·4			42·5			
"	"	" " 9	43·6	43·0	+0·6	43·7	43·1	B. G.	+0·7
7253	7313	" " 26		42·7	+0·3		42·9	G. B.	+0·5
7377	7437	" " 9	43·1			43·8			
"	"	" Oct. 4	42·1	42·6	+0·2	42·7	43·3	G. B.	+0·9
"	"	" " 7	42·7			43·4			
7437	7476	" Sept. 9	42·0						
"	"	" Oct. 4	42·7	42·6	+0·2				
"	"	" " 7	43·0						
7554	7593	" Sept. 26	40·3						
"	"	" Oct. 3	41·6						
"	"	" " 4	42·5	41·2	−1·2				
"	"	" " 7	40·3						
7607	7642	'58, Nov. 22		40·0	−2·4		40·8	B. R.	−1·6
7627	7683	'59, Sept. 26	41·4			42·7			
"	"	" Oct. 3	40·3	40·7	−1·7	41·5	42·0	A. B.	−0·4
"	"	" " 4	41·1			42·4			
"	"	" " 7	40·0			41·3			
7627	7749	" Sept. 26	40·0			41·5			
"	"	" Oct. 3	40·1			41·6			
"	"	" " 4	40·3	40·0	−2·4	41·7	41·5	A. G.	−0·9
"	"	" " 7	39·8			41·2			
7699	7807	'58, Nov. 22		40·1	−2·3				
7777	7800	" " 18	41·6			41·5			
"	"	'59, Oct. 4	43·1	42·2	−0·2	43·0	42·1	B. G.	−0·3
"	"	" " 7	42·0			41·9			
7843	7888	'58, Nov. 18	[49·0]			43·8			
"	"	" " 22	[46·7]	[48·1]	+[5·7]	41·5	42·9	R. G.	+0·5
"	"	'59, Oct. 4	[48·7]			43·5			
7856	7902	" Sept. 26		41·7	−0·7		42·6	R. G.	+0·2
7961	8032	'58, Nov. 18	43·6			42·7			
"	"	'59, Oct. 4	43·4	43·5	+1·1	42·5	42·6	B. G.	+0·2
7962	8023	" " 18		43·1	+0·7		42·6	G. G.	+0·2
7994	8023	" " 3	43·9			42·9			
"	"	" " 18	43·1	43·5	+1·1	42·1	42·5	B. G.	+0·1
7990	8169	'58, Nov. 8		43·6	+1·2		43·4	G. A.	+1·0
8079	8158	" " 18		44·2	+1·8		42·9	B. R.	+0·5
8177	8238	'59, Oct. 13	43·0			43·3			
"	"	" " 18	41·6	42·3	−0·1	42·0	42·7	G. G.	+0·3
8212	8237	'58, Nov. 18	42·9			43·4			
"	"	" " 22	42·9	42·9	+0·5	43·4	43·4	G. G.	+1·0
8282	8324	" " 18		41·7	−0·7				
8355	8	'59, Dec. 30		45·5	+3·1				
α Cyg.	61¹ Cyg.	" Oct. 3 to Nov. 25, 20 obs.		42·7	+0·3		42·5	A. A.	+0·1
γ Ceph.	ω Pisc.	" Oct. 3 to Dec. 24, 21 obs.		41·8	+0·4		41·7	A. A.	−0·7

If we divide these 92 pairs into four sets of 23 each, as indicated in the table by the lines after Nos. 5336, 6623, and 7119, the mean of each set respectively, and of the whole, will be as follows: column 1, giving the number of the set; col. 2, the number of results of pairs in the set by B. A. C.; col. 3, the mean of these results; col. 4, the number of results of pairs by other catalogues; col. 5, the mean of these; col. 6, the number of results by both B. A. C. and other catalogues taken together; col. 7, the mean of the same; col. 8, the probable error, *E*, of this mean; col. 9, the probable error, *E'*, of a single result for latitude.

	By B. A. C.		By other Catalogues.		By both B. A. C. and others.		E.	E'.
Set	Prs.	Mean.	Prs	Mean.	Num. of results.	Mean Lat.		
1	23	41°18′42″·46	20	41°18″42·34	43	41°18′42″·40	±0″·15	±0″·97
2	21	42 ·35	14	42 ·60	35	42 ·45	±0 ·14	±0 ·82
3	21	42 ·32	14	42 ·36	35	42 ·34	±0 ·17	±1 ·02
4	22	42 ·35	18	42 ·53	40	42 ·43	±0 ·12	±0 ·76
All	87	41 18 42 ·37	66	41 18 42 ·45	153	41 18 42 ·41	±0 ·07	±0 ·88
	E	± 0 ·10		± 0 ·10		Mean of the whole.		
	E'	± 0 ·97		± 0 ·80				

We may regard 41° 18′ 42″·41, then, as a close approximation to the latitude of the point in New Haven, where the observations were made, the probable error of this result, as given by the observations, being less than a tenth of a second. In taking the arithmetical mean of the results for the several pairs, we assume, of course, for each of these results equal weights—no discrimination having been attempted between errors of observation and of star-places, or between different catalogues. Such discrimination, however, in so large a number of independent pairs, would not have materially changed the result.

It will be seen, from the above table, that any one of the four sets would have given a latitude almost identical with the mean of the whole—in no case differing from it so much as a tenth of a second, or ten feet on the earth's surface. That this extremely close accordance, however, of results from sets composed of so few pairs, is in a measure accidental, may be inferred from the larger probable errors of these results as given in column *E*. Had there been three sets, instead of four, the differences would have been somewhat greater, though still not exceeding a few tenths of a second.

The probable error of a single result for latitude, as given in the last column, (about nine-tenths of a second on an average), includes, it will be understood, the accidental error of observation, (depending on observer, instrument, state of the atmosphere, etc.), together with the error of the stars' places, as given in the catalogues. The experience of the observers of the Coast

Survey with the zenith telescope, as stated by the superintendent, shows that usually the latter error very much exceeds the former, or that the catalogues are less reliable than the instruments. The same is apparent from our own results. While the probable error of a single result (including both error of observation and of catalogue) amounts, on an average to ± 0″·88, the probable error of observation, as distinct from that of catalogue, is found by comparing the repetitions on the same pairs in our first table, to be only ±0″·48. A similar value of this error is given by a comparison of the longer series of repetitions on the last two pairs of that table (α and 61¹ Cygni, γ Cephei and ω Piscium) as given below. Twenty observations were taken of the former pair, and twenty-one of the latter. The star-places used are those of the English Nautical Almanac.

Date of Obs.	α Cygni and 61¹ Cygni.	Δ	Date of Obs.	γ Cephei and ω Piscium.	Δ
1859.			'58, Nov. 8	41° 18′ 41″·2	−0″·5
Oct. 3	41° 18′ 42″·9	+0″·4	11	41 ·4	−0 ·3
4	42 ·0	−0 ·5	'59, Oct. 15	42 ·4	+0 ·7
7	41 ·9	−0 ·7	18	42 ·1	+0 ·4
10	41 ·9	−0 ·7	21	41 ·3	−0 ·4
11	43 ·7	+1 ·2	27	43 ·2	+1 ·5
15	41 ·8	−0 ·8	29	40 ·8	−0 ·9
18	42 ·7	+0 ·2	Nov. 1	42 ·0	+0 ·3
21	42 ·3	−0 ·2	3	41 ·4	−1 ·3
23	42 ·9	+0 ·3	14	43 ·3	+1 ·6
25	42 ·2	−0 ·4	15	40 ·6	+1 ·1
28	43 ·6	+1 ·0	23	42 ·4	+0 ·6
29	42 ·8	+0 ·2	28	41 ·7	0 ·0
Nov. 2	42 ·8	+0 ·2	Dec. 8	43 0	+1 ·3
3	43 ·4	+0 ·9	10	40 ·2	−1 ·5
4	43 ·0	+0 ·5	12	41 ·6	−0 ·1
9	43 ·2	+0 ·7	15	41 ·0	−0 ·7
14	43 ·0	+0 ·4	16	41 ·9	+0 ·2
17	41 ·7	−0 ·9	21	41 ·2	−0 ·5
19	42 ·0	−0 ·6	23	41 ·6	−0 ·1
25	41 ·3	−1 ·2	24	41 ·9	+0 ·2
Mean	41 18 42·55	± 0″·10	Mean	41 18 41·73	±0″·12
Prob. Er. of single result		+ 0 ·47			±0 ·55

We have, then, for the probable accidental error of *observation*, from the repetitions on the same pairs in our first table, ±0″·48; from α and 61¹ Cygni, ±0″·47; from γ Cephei and ω Piscium, 0″·55; from all these combined, ±0″·50. This compared with the average probable error of a single result for latitude, ±0″·88, in which it is included, gives for the probable error depending on the catalogues used, ±0″·72. The relative accuracy, therefore, of instrument and catalogues, in the present case, may be represented approximately by the ratio of 5 to 7. That the Twelve Year and other catalogues used are, on the whole, more accurate than that of the British Association, appears from the values of E' in the last line of the table on page 58; viz. ±0″·97 for B. A. C., and ±0″·80 for the other catalogues; although in the column for

the latter, as may be noticed, many of the positions for one of the stars of a pair, are also from B. A. C. If we separate from these values the probable error of observation, $\pm 0''.50$, before stated, we shall have for error depending on the catalogues, $\pm 0''.82$ for B. A. C., and $\pm 0''.63$ for the others. This result accords well with the remark of Prof. Bache, in a paper published in the 12th volume of the Proceedings of the American Association, that "the probable error of the result by a single pair of stars as depending on the catalogue errors of the stars' places, will range from $\pm 0''.60$ to $\pm 1''.00$."

In the same paper it is stated also, that "a probable error in observing of from $\pm 0''.25$ to $\pm 0''.80$ may be expected for one observation, according to the size and quality of the instrument and the ability of the observer." The average value of this error for some 300 pairs of stars, observed with the excellent instruments and by the experienced observers of the Coast Survey, is, according to the same paper, $\pm 0''.47$—ranging from $\pm 0''.25$ to $\pm 1''.14$. The value of the corresponding quantity in our own results, is, as we have seen, $\pm 0''.50$, ranging from $\pm 0''.47$ to $\pm 0''.55$. A considerable portion of this is due, doubtless, to the want of sufficient sensitiveness in the alidade level of the instrument; the run of which, as before stated, is only one thirteenth of an inch for 5″, smaller portions of the scale than this being measured by the eye. This level, we may remark, was intended originally only to be used in the ordinary way, in connection with the graduated circle, and is sufficiently sensitive for that purpose; but not enough so to correspond, in point of accuracy, with the micrometer,* or for the delicacy required in observations for latitude with the zenith telescope. The use of the instrument for such observations was wholly an after thought, and no change was made to adapt it to its new functions. It is of smaller size also than the zenith telescopes of the Coast Survey, and it may be proper to add, that, being of the observer's own make, and a first attempt of the kind, some allowance is due in a comparison of its results with those of more finished instruments in the hands of trained observers.

In order to compare the latitude, thus determined astronomically, with the geodetical determination of the Coast Survey, a survey was made with theodolite and chain connecting the writer's observatory with three points in the city, the positions of which are given in the Coast Survey Report for 1851, viz., "Brewster's Factory Cupola" at the foot of Wooster street, "Episcopal church," which means the west spire of St. Paul's,

* Sixty successive bisections of Polaris, near culmination, with the micrometer wire, gave for probable error of a single bisection, $\pm 0''.30$; which would make the probable error of a single result for latitude (involving two bisections) as depending on the micrometer alone, $\pm 0''.21$.

and "College Spire," which is that of the Lyceum, or middle spire. The results are as follows.

	Lat. by Talcott's method.	By Coast Survey.	Diff.
Observatory of the writer,	41° 18′ 42″·41		
Factory Cupola, East Street,	3·29	41° 18′ 7″·10	3″·81
St. Paul's Church, W. Spire,	7·52	11·32	3·80
College Observatory, (Athen. Tower),	22.36		
"College Spire," (Lyceum),	23·90	27·74	3·84
Chapel Spire,	25·49		

Mean excess of geodetic over astronomical lat. 3″·82.

It may be added that a series of observations made in 1850, with an excellent Pistor and Martins' Patent Sextant, on northern and southern stars, and also a series of observations made in 1858, in the usual way, with the circle of the Transit instrument before described, on Polaris and other stars, both direct and by reflection, and in reversed positions of the instrument, gave results differing but about half a second from the above determinations, the former minus, the latter plus.

The differences among results for latitude, depending on differences of catalogues, are often considerable, even when the best authorities only are trusted; as will be seen by comparing the mean results, given below, of the observations on α and 61^1 Cygni, and γ Cephei and ω Piscium, according as the declinations are taken from the several catalogues named.

	α and 61^1 Cygni.	γ Cephei, ω Piscium.
English Nautical Almanac,	41° 18′ 42″·5	41° 18′ 41″·7
Twelve Year Catalogue,	42·6	41·4
B. A. C.,	42·7	41·8
Greenwich, 1850,	42·8	41·6
Washington, 1845–1850,	42·7	
Radcliffe, 1856, '57,	41.9	40·3
Bradley,		40·7
Piazzi,		41·1
Argelander,		41·8
Mädler,		41·4

It will be noticed, also, in respect to these two pairs, that there is a greater difference between their respective mean results, than we should expect from well determined nautical almanac stars. This difference between the means of the twenty and twenty one observations respectively on the two pairs, using the nautical almanac declinations, amounts to 0″·8, an amount difficult to attribute to any errors of instrument or of observation, and equally difficult, perhaps, to ascribe to errors of stars' places, especially as the several catalogues so well agree with each other. From whatever source it may arise, it seems to be constant, and will receive further investigation.

ART. VIII.—*On Fixing Magnetic Phantoms;* by Prof. J. NICKLÈS.

THE name phantom was given by M. de Haldat* to the figures which are obtained when iron filings are thrown upon a sheet of paper or a pane of glass placed over a magnet. This physicist fixed these images by producing them upon a sheet of paper coated with starch or prepared with gelatine.

This process certainly enables us to obtain the general form of the phantoms, but all physicists can see that it suppresses the details. I was more particularly struck with this fact on a recent occasion, where I sought to fix the phantoms of some new electro-magnetic combinations; I therefore propose another method, which is here briefly given; it is very simple and succeeds perfectly. The paper upon which the phantoms are to be fixed is "waxed" paper. A sheet of this is placed over the poles of the magnet in question, and kept in a horizontal position by means of a screen placed between the paper and the magnet. Then proceeding in the usual manner, when the image is fully developed, a hot brick is held above it, or the warm lid of a crucible, which is preferable because it is lighter, and easily managed with the tongs. They must not touch the paper, but only be brought within the distance necessary to fuse the wax. As soon as this happens, which is easily perceived by the glistening appearance produced, the brick is withdrawn. Meanwhile the current does not cease its activity, nor the filings lose their arrangement, in which position the whole solidifies so well that the fixed image does not at all differ from the phantom of the magnet in activity.

This result is explained as follows. By capillarity the melted wax penetrates the masses of filings, very much as water penetrates a heap of sand; the heat of the brick facilitates this, by preventing the solidification of the wax, and as the temperature is not sufficiently elevated to sensibly affect the magnetism developed, the phantom, after the solidification, and in the most minute details, preserves the same arrangement which the iron filings had, while they were free to obey the action of the magnet.

A condition indispensable to success, is, that the stratum of wax has a sensible thickness, so that it may suffice for the agglomerations, since these absorb melted fatty matter, even to saturation. That this force of absorption is very energetically exercised, may be perceived after the cooling, since the paper about the agglomerations is deprived of wax, and differs thus in appearance from those parts where capillarity has not been exercised. It is therefore possible to preserve to the phantoms the

* Memoir before the Academy of Stanislas, p. 43, for the year 1839—1840.

relief which has before been sought in vain, and what will be still more useful, to give permanence to the sort of molecular arrangement which the filings take, when exposed to magnetic influence.

Instruction can hardly fail to be derived from the use of these means, by aid of which it will be possible to study the figures more advantageously, which are, in some sense, the visible expression of the force animating bodies endued with polarity developed by magnetism.*

Nancy, March, 1860.

ART. IX.—*On some Questions concerning the Coal Formations of North America;* by LEO LESQUEREUX. Continued from Vol. xxviii, p. 21.

Geographical Distribution of the Coal Flora.

To follow the plan exposed in the first part of this memoir (Silliman's Journal, No. 82, July, 1859, p. 21) I should have to examine now the nature of the coal-flora, that is, the anatomical and chemical constitution of the coal plants, first comparing between themselves the groups of species of plants from which the matter of the coal is a compound, and then examining how this vegetation is related to the plants living at this epoch. The distribution of the coal plants, either geographical or stratigraphical, is a question accessory to the former. Nevertheless, it has become now of a greater importance, since it touches upon a problem which is at present discussed by the authority of the highest scientific names. I allude to the theory *of the origin of species* by Mr. Darwin. It evidently concerns the great problem of the inmost nature of man, and thus forces every naturalist to seek, in the sum of facts gathered up by his researches, either confirmatory or contradictory evidence of views which cannot but preoccupy his mind. Thus it is apparently advisable to change the order of examination of the flora of the coal measures of North America, studying it now in its stratigraphical and geographical distribution, and leaving for another opportunity the discussion concerning the nature of its vegetation and the specific and generic value of its representatives.

* This note is extracted from a work now in press and which will soon appear, entitled, *Electro-magnets and Magnetic adhesion*, and accordingly treats of electro-magnets and their application to locomotion upon railways, with the design of increasing the adhesion of locomotive engines. To the experiments which we made public in 1853 (v. this Jour., [2], vol. xvi, p. 337), we shall add new ones which allow us to expect a speedy solution of this problem, so important to railroads.

In regard to electro-magnets, we will only say at present, that we shall mention a great number of new species, which have compelled us to invent a new classification and a nomenclature based upon the principles of the natural method. We have also to make known a great number of new facts respecting the laws and properties of electro-magnets, to say nothing of what we wrote upon this point in 1853 (v. Am. Jour., [2] vol. xv, p. 381, and vol. xx, p. 100). J. N.

I shall not attempt in any way either an exposition or a critical examination of the views of the celebrated English author. This task has already been admirably fulfilled in a former number of this Journal.* I shall merely expose the facts that appear surely ascertained by a long and careful exploration of the coal-fields of North America, leaving the naturalist-philosopher to take from these facts any conclusion that may appear just to him. It is a mite only. But the monuments of humanity, like the mountains of limestone, are built by the slow accumulation of minute remains.

The botanical palæontology of the coal-period and the succession and variation of species in the different strata of the coal-measures, cannot be studied with more advantage and with more chances of reliability than in the coal-fields of the United States. Their immense area, the uniformity of their generally unbroken stratification, the facility of ascertaining in many localities the order of this stratification, the numerous exposures of coal banks, not limited to a particular district, but opened and worked now at distant points over the whole area of the coal-fields; all this affords to a systematic exploration such advantages as cannot be found in any other country of the world.

Moreover, my explorations of the coal-fields of North America have been favored by peculiar circumstances. Connected at different times, during ten years, with the geological surveys of the states of Pennsylvania, Kentucky, Indiana, Arkansas and Illinois; called to survey for comparison the coal-fields of Ohio and part of those of Virginia; constantly limiting my researches to botanical palæontology, I have thus, I suppose, collected on the distribution of the coal-plants in North America, more materials and more reliable accounts than geologists may be able to get at for a long time to come.

The first important question in regard to the coal-plants of America is: what is their relation of forms with the plants of the same formation in Europe? The comparison of the coal flora of both continents has never been made except on few and insufficient data.†

Except two, all the genera of fossil plants of the coal-fields of America are represented in Europe. One of them is the remarkable *Whittleseya elegans* of Dr. Newbury, a flabellate, apparently short pedicillate, cuneiform-oval and truncate leaf, found hitherto always detached from the stem. It evidently differs from the genus *Cyclopteris* by its simple straight nervation and by its up-

* Vol. xxix, p. 1.

† I have attempted it formerly, in my palæontological report of the Geological State Survey of Penn., delivered January 1st, 1854, but only published five years afterwards. As I have been denied the privilege of reviewing and correcting my manuscript before its publication, I cannot consider myself accountable for the errors, and especially for the deficiency of conclusive data, which may be found in it.

per margin being horizontally truncate and regularly wavy-denticulate by the percurrent and slightly emerging nerves. The typical affinity of this plant is unknown. It is perhaps more related to *Cordaites* or even to *Salisburia* than to a fern.

The second genus peculiar to the American coal-flora is my *Scolopendrites*, represented, like the former, by a single species; *Scolopendrites dentata* Lsqx., of which fragments only have been found. The name has no relation to the nervation of the leaf, but to its outline. This leaf is apparently five to six inches long, more than an inch broad, lanceolate, deeply cut by obtuse somewhat regular teeth and marked by a few very thin distant nervules, emerging from a narrow medial nerve in an acute angle, scarcely arched and forking twice. Both these genera represent peculiar types to which no form of the European coal-flora can be compared. I could also mention as a peculiar type of our coal-flora *Crematopteris Pennsylvanica* Lsqx., a thick stem or branch, on both sides of which short, lanceolate, obtuse, thick leaflets, without any trace of nervation, are pinnately attached. The single specimen which I have found of this plant was not well preserved enough, and it would be unsafe to consider the species which it represents as having been seen in its true form and full development.

If we admit the generic distribution of the fossil plants of the coal, as it has been established by Brongniart in his *Tableau des Genres,* (certainly the best that has been attempted either before or after him) all the European genera, even the undefined genus *Aphlebia*, (Sterbg.) have representative species in the coal-fields of America. From the nomenclature of Göppert and Corda, a few European genera, it is true, are hitherto without representative species in our coal-fields. But these genera, established on the form and the position of fructification, always very difficult to identify from even the best preserved specimens of fossil ferns, may be represented by some species of our *Sphenopterideæ* and *Pecopterideæ*. These genera, viz., *Rhodea* Sternb., *Trichomanites* Göpp., *Steffensia* Göpp., *Beinertia* Göpp., *Diplaxites* Göpp., *Woodwartites* Göpp., are no peculiar types. Thus, considering their generic distribution, the coal-plants of Europe and of North America show very little difference indeed. But in examining the species separately and comparing them on both sides of the Atlantic, the number of forms peculiar to America appears much larger than it was at first supposed, and thus the vegetation of our continent, at the epoch of the coal formation and considered in its whole, is far more different than it has been supposed heretofore.

It would be too long and tedious, perhaps, to take one by one and compare all the American species with those of Europe,

mentioning the identity, the relation, or the difference of each of them. I will therefore give in a table the number of species of each genus, belonging either to North America or to Europe, or common to both continents, and complete this general view by a few remarks on some of the species, apparently the most interesting, by predominance of number, of typical form or by a peculiar distribution.

Genera of Coal plants.	Species peculiar to America.*	Species peculiar to Europe.	Species common to both.
1. Noeggerathia Sternb.,	3 + 2*	5	1
2. Cyclopteris Brgt.,	1	2	2
3. Nephropteris Brgt.,	5	4	4
5. Neuropteris Brgt.,	18 + 1*	16	12
6. Odontopteris Brgt.,	4 + 1*	6	3
7. Dictyopteris Gutb.,	1	1	0
8. Sphenopteris Brgt.,	10 + 9*	41	12
9. Hymenophyllites Göpp.,	6	10	2
10. Rhodea Sternb.,	0	1	0
11. Trichomanites Göpp.,	0	4	0
12. Steffensia Göpp.,	0	1	0
13. Beinertia Göpp.,	0	1	0
13. Diplaxites Göpp.,	0	2	0
14. Woodwartites Göpp.,	0	2	0
15. Alethopteris Sternb.,	9 + 2*	20	9
16. Callipteris Brgt.,	2	1	1
17. Tecopteris Brgt.,	12 + 4*	49	12
18. Aphlebia Sternb.,	0	6	1
19. Caulopteris Brgt.,	4	4	0
20. Psaronius Brgt.,	10	6	0
21. Crematopteris Schp.,	1	0	0
22. Scolopendrites Lsqx.,	1	0	0
23. Whittleseya Newb.,	1	0	0
24. Cordaites Ung.,	1	0	2
25. Diplothegium Corda,	0	0	1
26. Stigmaria Brgt.,	5	2	5
27. Sigillaria Brgt.,	12 + 9*	37	17
28. Syrigodendron Brgt.,	0 + 1*	0	2
29. Diploxylon Corda,	0	0	1
30. Lepidodendron Brgt.,	14	10	11
31. Ulodendron Rhode,	0	4	2
32. Megaphytum Artis,	1 + 1*	4	0
33. Knorria Sternb.,	2 + 2*	1	2
34. Halonia Ll. & Hutt.,	0	2	1
35. Lepidophyllum Brgt.,	7	2	4
36. Lepidostrobus Brgt.,	?	1	2

* In this enumeration, I count the species named as new ones in a catalogue published in 1853 by Dr. Newberry (Nos. 8th and 9th of the Annals of Science of Cleveland). As the species have not been described and did not come under my examination, they are still doubtful and separately marked by a *.

Genera of Coal plants.	Species peculiar to America.	Species peculiar to Europe.	Species common to both.
37. Cardiocarpon Brgt.,	2 + 5*	6	0
38. Trigonocarpum Brgt.,	3 + 3*	5	5
39. Rhabdocarpos Göpp. & Brgt.,	1 + 1*	6	1
40. Carpolithes Sternb.,	12 + 1*	52	6
41. Selaginites Brgt.,	0	1	0
42. Lycopodites Brgt.,	1	12	0
43. Lomatophloios Corda,	1	0	1
44. Lepidophloios Sternb.,	0	0	1
45. Bothrodendron Göpp.,	0	1	0
46. Cycadloidea Buckl.,	0	1	0
47. Calamites Suck.,	2	5	11
48. Bornia Sternb. & Göpp.,	1	1	0
49. Equisetites Sternb.,	0	2	1
50. Asterophyllites Brgt.,	5	8	7
51. Annularia Sternb.,	0	0	5
52. Sphenophyllum Brgt.,	2 + 3*	3	3

Noeggerathia Sternb.—Two of the American species are closely related to *N. obliqua* Göpp. A third, *N. Bocksiana* Lsqx., which I referred to *Cyclopteris Bocksiana* Göpp., from the exact likeness of its terminal leaflet with the separate and only leaf on which the author has established his species, is evidently referable to the genus *Adiantites* Göpp. The two species of true *Noeggerathia* belong to the Old Red Sandstone of Pennsylvania; the third found at the base of the millstone grit of Pennsylvania belongs by its typical form to the true coal measures.

Nephropteris Brgt.—Cannot be separated from the genus *Neuropteris*, since it represents large, mostly rounded and deciduous leaflets attached around the stem at the base of secondary pinnæ of some *Neuropteris*. Two of our American species, viz., *Nephropteris fimbriata* Lsqx. and *Nephropteris laciniata* Lsqx. have a typical character which has never been seen on any of the fossil ferns of Europe. As the names indicate, both these beautiful species of leaves are fringed and laciniate on their circumference. The fringes and laciniæ are unequal in length and breadth, flexuous, and do not bear any likeness to the straight and regular points surrounding the fruit-bearing leaves of some ferns of our time.

Neuropteris Brgt.—Our *Neuropteris hirsuta* Lsqx. is, probably at least, the equivalent of *Neuropteris cordata*, *N. Scheuxeri*, *N. angustifolia* and *N. acutifolia*, species of M. Brongniart. A peculiar character of this species, viz., the long, straight, diffuse hairs of its upper surface may help to bring together the numerous and diverse forms of its leaflets, which are generally separated from the stem. Mr. Bunbury had already remarked and mentioned those hairs. I have not been able to detect them on the few, badly preserved specimens of the European *Neuropteris cor-*

data Brgt. which I had opportunity to examine. It may be also that *Neuropteris smilacifolia* Sternb., *N. plicata* Sternb., *N. rotundifolia* Brgt., ought to be referred as varieties to *Neuropteris flexuosa* Brgt. Nevertheless, we have these same scarcely distinct species with just the same characters in America. They are mostly found in the same places where *Neuropteris flexuosa* abounds. Of the other American species of this genus, *Neuropteris Clarksoni* Lsqx. is closely allied to *Neuropteris auriculata* Brgt., and *Neuropteris Desorii* Lsqx., to *Odontopteris Reichiana* Gutb. But *Neuropteris speciosa* Lsqx., *N. rarinervis* Bunby., *N. gibbosa* Lsqx., *N. undans* Lsqx., *N. dentata* Lsqx., *N. Morii* Lsqx. are widely different from any European species, and may be considered as true American forms or types. *Neuropteris adiantites* Lsqx. should perhaps be referred, by its peculiar nervation, to the genus *Sonopteris* of M. Ponsel.

Odontopteris Brgt.—I have counted *Odontopteris Brardii* Brgt., among the species belonging to both continents, on the authority of M. Unger, who indicates it at Mauch-Chunk, Pennsylvania. Nevertheless I doubt of the identity of our species with the true *O. Brardii* Brgt. The small specimen which I formerly referred to it belongs to another species, *Odontopteris crenulata* Brgt., and I would rather suppose that some incomplete specimen of *Odontopteris alata* Lsqx., has been mistaken for the true *O. Brardii* Brgt. Our species, though closely related to the European form, evidently differs from it by its obtuse leaflets, and by the position of two large, opposite, cuneate, truncate leaflets attached to the rachis just below the base of the pinnæ.—*Odontopteris Schlotheimii* Brgt., common to both continents, is one of the few species which, either in its fructified or sterile form, show a perfect identity at any locality where it is found.

Dictyopteris Gutb.—The affinity of the European *Dictyopteris Brongnarti* Gutb. with our *Dictyopteris obliqua* Bunby., is so great that these species could be considered as mere varieties of the same. *D. obliqua* Bunby. has narrower, less obtuse leaflets. These are generally found detached from the stem and spread over the shales in the greatest abundance.

Sphenopteris Brgt.—This genus, to which I have united the *Gleichenites* of M. Göppert, appears from the number of its species, to be far more abundantly represented in Europe than in America. Probably the difference may be accounted for, first by the number of European species which, established on small specimens, represent variable parts of a frond and may be recalled to a common species. Secondly by the insufficiency of our explorations. Since the publication of my catalogue of fossil plants of the coal, in 1858, ten new species of *Sphenopteris* not counted in the table, because they are still undescribed, have come under my examination. From the number of species enu-

merated by Dr. Newberry it would appear also that this genus is largely represented at some places and in restricted areas. Thus we may expect to see the number of American species largely increased.

The essential European types of this genus are all represented in our coal measures, either by identical species, or, by closely related forms. Thus we have: *Sphenopteris spinosa* Ll. and Hutt., *S. tridactylites* Brgt, *S. Davalliana* Göpp., *Sphenopteris irregularis* Gutb., *S. Gravenhorstii* Brgt., *S. Dubuissonis* Brgt., *S. latifolia* Brgt., *S. polyphylla* Ll. and Hutt., *S. artemisiæfolia* Sternb., &c. Besides, as American types, we have *Sphenopteris flagellaris* Lsqx., and especially *Sp. Newberrei* Lsqx. This last species has a ramification different from that of any other kind of fossil fern, viz., two secondary pinnæ, joined at their base and forking in an obtuse angle at the top of a short naked stem. The form of the leaflet and the nervation of this species is also peculiar. One of my species, *Sphenopteris decipiens* Lsqx. has, like *Neuropteris adiantites* Lsqx., the primary nerve folded along the base of the leaflets and the nervules curved and branching upwards, a character ascribed by Brongniart to the genus *Lonopteris* Tom.

Hymenophyllites Göpp.—From analogy of nervation, rather than from a relation of typical form, I have, perhaps wrongly, connected with this genus, some species referable either to *Aphlebia* Sternb., or to *Schizopteris* Brgt., or even to *Selaginites* Brgt. Four American species: *Hymenophyllites fimbriatus* Lsqx., *H. affinis* Lsqx., *H. hirsutus* Lsqx., *H. laceratus* Lsqx., are represented by fronds which form a broad base, divide in ascending, and are thus irregularly cut in simple, ordinarily short, curved, somewhat obtuse laciniæ or lobes. Each of the divisions is marked by a single nerve, ascending to its top. The fronds appear generally of a thick texture; but in *H. giganteus* Lsqx., which may be the same plant as *Schizopteris lactuca* Sternb., they are seemingly very thin. These species ought to constitute a separate genus. In the fossil flora of Pennsylvania I had attempted to group them together under the name of *Pachyphyllum;* but as some species have apparently thin leaves or fronds, the name could not be preserved of course. A discussion concerning the morphology of the plants of the coal would be out of place now. The subject ought to be separately treated. I needed only to mention these peculiar forms, for comparing the distribution of the genus *Hymenophyllites.* Two of our American species are closely related to the *H. elegans* Brgt. of Europe. The others, especially *H. fimbriatus* Lsqx., are apparently peculiar American types. This last, nevertheless, could be compared to *Selaginites Erdmani* Gutb.

Alethopteris Sternb.—All our American species have some affinity with European types of the same genus. Even the re-

markable *Alethopteris serrula* Lsqx. is related, though distantly, to *A. erosa* Gutb., from which it differs, especially by its very long (sometimes 4 to 5 inches), narrow, linear pinnæ. It has been found only near Pottsville, Pennsylvania.

Callipteris Brgt.—One of the American species, *C. Sullivantii* Lsqx., is a beautiful fern of a peculiar character, apparently bi- or tri-pinnate, but of which pinnæ only have been found. The general outline of the pinnæ is like that of *Alethopteris Serlii* Brgt. But the pinnules are twice as broad, rounded at the top, marked by a broad and deep medial nerve, abruptly terminating in the middle of the leaflets, and from which emerge in acute angles, thin arched, forked nervules as closely placed as those of *Neuropteris flexuosa* Brgt. The other American species is closely related to *Alethopteris sinuata* Brgt. *A. nervosa* Brgt. is a common species over the whole extent of our coal fields. As it is generally found connected (though not on the same stem) with *A. Sauveurei* Göpp. (*A. nervosa* var. Brgt.), both forms are referable to the same species.

Pecopteris Brgt.—The identification of the species of this genus is difficult. Here, as with the *Sphenopterideæ*, some species have been established on fructifications, which are seldom found in such a state of preservation that the form and the position of the fruit-dots can be ascertained, and others have been differently named from the branching of the veinlets, which differs in the same species according to the position of the leaflets. Thus identical species have received different names or even been placed in different genera. The number marked in the table represents only species of ours of which the value is ascertained. Some of them show apparently true American types. Thus, *Pecopteris Sillimani* Brgt., *P. Loschii* Brgt., *P. Velutina* Lsqx., *P. distans* Lsqx., *P. decurrens* Lsqx., and *Pecopteris concinna* Lsqx., widely differ from any European fossil *Pecopteridee* known till now.

Caulopteris Brgt.—Uniting *Stemmatopteris* Corda, to this genus, we have at least four distinct species.

Stigmaria.—I think that the varieties of *Stigmaria anabathra* Corda, described by M. Göppert, are true species, being generally found in our coal-fields at different geological horizons. The five species described as new in the report of the Pennsylvania survey, may be perhaps reduced to three, but some well characterized species have been found since the report was made.

Sigillaria Brgt.—The distribution of this genus does not operate identically on the coal-fields of both continents. We have few species of the narrow-costate *Sigillariæ*, more than 30 species of which are counted in Europe. We have more of the broad-costate forms and especially a large number of species belonging to the peculiar section of the smooth or rather uncostate *Sigillariæ*.

In this last section, three well characterized forms are peculiar to our coal-fields. The one has its surface stellately wrinkled around the scars; the second is wrinkled crosswise, and the third has double, oval, obliquely-placed scars united by a deep groove, and the surface is beautifully reticulated by narrow wrinkles, obliquely intersecting each other. The beautiful *Sigillaria Schimperi* Lsqx., of which the large scars have nearly the form of an eye, is also a peculiar American form. The number of our species, as marked on the table, is too small. About ten new species belonging to the Geological State surveys of Kentucky, of Arkansas and of Illinois are not here counted.

Syrigodendron Brgt.—The two species described by Mr. Brongniart are common in America. I have never seen any other. Dr. Newberry indicates a peculiar species of ours under the name of *S. Americanum* Newb.

Lepidodendron Sternb.—The great number of specimens of this genus collected from the base and the top of the millstone-grit series of our coal measures, has afforded a good opportunity for examining the development and variations of the scars at the different stages of growth of the trees. Except *L. oculatum* Lsqx., which might be referred perhaps to *L. distans* Lsqx., all the new species described in the report of the survey of Pennsylvania are well characterized. Some of them might be considered as American types. *L. distans* Lsqx., is related, by the distance of the scars only, to *Sagenaria rimosa* Sternb.

Megaphytum Artis.—A beautiful and large specimen of a new species of this genus is preserved in the cabinet of the Geological State Survey of Illinois. Another new species is mentioned by Dr. Newberry under the name of *M. discretum* Newb.

Knorria Sternb.—The number of American species is apparently pretty large; but the difficulty of determining the species from specimens generally badly preserved has prevented or retarded the descriptions. *Knorria imbricata* Sternb., is especially common below the millstone grit.

Lepidophyllum Brgt.—We have already seven well characterized American species, and three new and unpublished ones. The number of peculiar forms of these scales or leaves of the cones of *Lepidodendron*, evidently shows that the large proportion of species of *Lepidodendron*, which have been found in America, cannot be considered as resulting from peculiar changes of the same species, according to the age of the scars. *Lepidophyllum hastatum* Lsqx., *L. brevifolium* Lsqx., and *L. plicatum* Lsqx., are American types not related to any European species.

Lepidostrobus Brgt.—The number of cones of *Lepidodendron* is extremely large, especially in the shales of the first bed of coal above the conglomerate. All the species ascertained, from the

form of their scales, apparently the only reliable specific character for a determination, are counted with *Lepidophyllum*.

The fruits distributed among the genera *Cardiocarpum* Brgt., *Rhabdocarpos* Göpp. and Berg., *Trigonocarpum* Brgt., *Carpolithes* Sternb., are generally found broken or flattened or divested of their outer envelope. Consequently the identification of the species is very difficult. The number of these fruits disseminated in the shales and in the sandstone of the low coal of our American basin is considerable. They follow the stratigraphical distribution of the genus *Lepidodendron*, though they do not appear in any way related to this genus. About twenty well marked but undescribed species ought to be added to those counted in the table. Some of them have peculiar forms without relation to any European species. One of the most remarkable, *Rhabdocarpos arcuatus* Lsqx., is described and figured for the fourth volume of the Geological Report of the State Survey of Kentucky, under the direction of Dr. D. Dale Owen.

Lycopodites Brgt.—Nothing, perhaps, shows more evidently the difference in the characters of the coal flora of both continents than the scarcity of species of *Lycopodites* and the abundance of species of *Lepidodendron*, in our coal measures; when a contrary distribution rather predominates in Europe. Both the genera are considered as closely related. Nevertheless we have a single species of *Lycopodites*, very rare indeed, since I found specimens of it only lately on both the extreme limits of the state of Kentucky, at the same geological horizon, viz., in the shale of coal No. 1B. By its concave leaves, decurrent and embracing at the base, it differs from any of the twelve European species of the coal yet described Dr. Newberry has not mentioned any species of this genus in his catalogue.

Asterophyllites Brgt.—As some species of this genus are represented by catkins, scarcely if ever found attached to the stems, and of which therefore the relation is obscure or unknown, I may have counted as peculiar to America a few species which are only fruiting modifications of some others. I consider those fruits as male catkins, attached to large branches, while the true fruit-bearing catkins, which are much smaller, were apparently terminating the branchlets;—a kind of fructification somewhat analogous, but in a position contrary to that of some pines.

Without including the species of Dr. Newberry, the table of distribution shows that from six hundred and fifty-five species of coal plants now determined; more than one hundred and sixty are peculiar to America; three hundred and fifty species are known only in Europe; and one hundred and fifty are common to both. It is certain that future researches will greatly add to the number of species common to both continents, but as much, also, to the number of species peculiar to America. There-

fore, the difference pointed out by the table, may be admitted as fairly representing, in a proportional manner, the general distribution of the coal plants on both continents. The botanical remains of the coal-fields of Europe have been carefully collected and studied by learned naturalists for more than a century, while those of America are only beginning to be noticed by scientific explorers.

In the Introduction to the Fossil Flora of the coal-fields of Pennsylvania, I had already pointed out the great analogy existing between the plants now living on the peat-bogs of America and of Europe. Admitting the peat-formation of our time as being the actual representative of the coal-marshes of the coal-epoch, I was led to the conclusion: that at this last epoch, the flora of both continents was as different and even more different than its representative flora is at our time. Thus, on twenty-five species of mosses growing on the peat-bogs and entering into the formation and composition of the peat, a single one is peculiar to North America. By extending my researches to the South, namely to the latitude of Norfolk, in the great Dismal Swamp of Virginia, I found the proportion changed in some manner; but nevertheless, the most common forms of the genus *Sphagnum*, which in Europe and North America form the principal mass of the peat, were found there also, performing the same work in the composition of the combustible matter. In the family of the Ferns, out of ten species growing on our marshes, five are identical with European species growing in the same situations; and two more are so closely allied to their European congeners that in a state of petrifaction, they could not be distinguished from each other. Even now, in their normal state, they are admitted by some botanists as varieties only. Among the *Junceæ*, *Cyperaceæ* and *Gramineæ*, twenty-six species out of forty-one are common to both continents; and from the other families, of which representatives are found on the peat-bogs, twenty-six species of thirty-one are found in Europe and in America. The likeness of some species of this section, peculiar to both continents, is still remarkable. Thus, *Larix Americana* and *Larix Europœa*; *Nymphœa odorata* and *Nymphœa alba*; *Ledum latifolium* and *Ledum palustre*, *Trientalis Americana* and *Trientalis Europœa*; *Vaccinium macrocarpum* and *V. Oxycoccos* (many others still could be named), are so nearly related that their specific characters can be distingnished only on good and complete specimens. There are, indeed, on the peat-bogs of America, some peculiar types which are not found in Europe: *Xyris bulbosa*, *Taxodium distichium*, *Sarracenia purpurea*, and a few others. But it is even so with the plants of the coal-period where we have seen certain types peculiar to this continent. This peculiarity serves only to render the more strik-

ing the analogy of distribution of the flora of both epochs. It shows the same degree of difference and of analogy. Some species, even a few types, peculiar to each country, the greatest number of them peculiar to America; many identical species, and especially many forms, so nearly related, that it becomes very difficult to separate them by specific characters.

Columbus, Ohio, April, 1860.

(To be continued.)

ART. X.—*On an Oil-Coal found near Pictou, Nova Scotia; and the Comparative Composition of the Minerals often included in the term Coals;* by HENRY HOW, Prof. Chemistry and Nat. Hist., King's College, Windsor, N. S.

THE name given to the substance I purpose describing indicates the use to which it is put, viz., the manufacture of paraffine-oil, and an inquiry into the association of elements in the minerals constituting the sources of this and similar "mineral-oils" and in the bituminous coals, may possess some interest in a chemical point of view. As regards the classification of these it is not necessary to do more than recall the attempt made a few years ago in courts of law, in Scotland, New Brunswick and Nova Scotia, to decide what should and what should not be called a coal. The great array of evidence of various kinds brought to bear on the question rendered it a very interesting one, and it is well known that the opinions of the numerous scientific men consulted on these occasions were so nearly balanced that the point at issue was determined on the commercial, rather than on the scientific merits of the cases. It will be remembered that the substances in dispute were the Torbane-hill "coal," found near Bathgate in Linlithgowshire, Scotland, and the Albert "coal," occurring at Hillsborough, New Brunswick. As respects the former, the result of the trial in Edinburgh, in 1853, was that the jury considered it to be "coal, in the common sense of that word;" and, as regards the latter, it was decided at Fredericton, N. B., 1852, and at Halifax, N. S., to be also a "coal." Notwithstanding these legal decisions, which, from the conflicting opinions of witnesses, were obviously arrived at from other than scientific considerations, the question as to what is and what is not a coal, must be held to be an open one in those sciences in whose province the matter lies; and it will probably long remain so, because it was not from the absence of data, but from differing interpretations of facts about which for the most part there was a general accordance, that there arose the well known want of unanimity among geologists, mineralogists, chemists and microscopists.

In this paper I do not hope to decide the question, but I wish to point out as interesting facts the occurrence of true bituminous coal in contact with the Oil-Coal, and to call attention to the relative proportions of the ultimate elements in the latter and in the before-mentioned disputed substances as compared with bituminous coals, as important in explaining the different nature of their products of distillation, and in affording support to those who do not make one species only of these minerals.

Some of the analyses which follow are published for the first time; others, of my own, relating to bituminous coals, I have taken from among those given in the Report on Coals suited to the Steam Navy of Great Britain, by Sir H. Delabeche and Dr. Playfair, and those of cannel-coals are taken from sources hereafter indicated.

The oil-coal found near Pictou, N. S., was first met with by persons residing in the neighborhood, early in 1859, and its exact locality is called Fraser Mine. I occurs in the coal-measures. I am indebted to Henry Poole, Esq., manager of the Fraser Mine, for the following particulars relating to the geological position, etc., of the substance:—

"The lowest measures about sixty yards, on the surface, short of the distance where the oil-coal crops, are composed chiefly of strong bands of sandstone, actual thickness not yet proved; then shales with bands of ironstone, and Stigmaria roots with Sigillaria stems, and a few detached fern leaves, in such soft shale that I have not been able to preserve any good specimens. Immediately above the oil-coal is a seam of bituminous coal about fourteen inches thick. Where we commenced to open a mine by driving a slope, the oil-coal was fourteen inches thick, but at 200 feet down at the bottom of the slope the oil-coal was twenty inches thick; it has a smooth regular parting at top next the coal, as also at the bottom next to the Oil-Batt below, but throughout its entire thickness it is of a curly twisted structure; many of its fractures look like the casts of shells, and the sharp edges are polished of a "slickensides" character. No fossils that I am aware of have hitherto been found in the curly Oil-Coal. The Oil-Batt next below is nearly two feet thick, of a homogeneous character with a slaty cleavage of various thicknesses. In this band two or three varieties (species?) of Lepidodendron beautifully preserved have been found, also leaves about one-fourth of an inch wide, and in lengths of from four to six inches, which have undergone so little change, that when the damp shale was fresh split, they could be removed, and were so elastic that they could be bent considerably without breaking. At the bottom of the slope another thin seam of curly Oil-Coal has appeared of a few inches in thickness, but is not worked at present. In the roof-coal were found pieces of de-

cayed wood very little changed, which I consider a great curiosity. On M'Lellan's Brook shale is above the Oil-Coal, and Oil-Batt below in which have been found Lepidodendra and apparently molar teeth with three fangs, flattened modiola shells, and spines or small fish-teeth. The Oil-Batt has been found in several places without the curly band or so-called Oil-Coal. Two thousand tons of Oil-Coal have been raised (Dec. 1859) at the Fraser Mine."

The Oil-Coal varies in color from brown to black, is dull where not polished as just mentioned, has a reddish-brown lustreless streak, its powder is dark chocolate colored, it is very tough and breaks at last with a hackly fracture, its specific gravity, in mass, after the vessel of water containing it had been in an exhausted receiver =1·103. It takes fire very readily, and when removed from the lamp still burns for some time with a brilliant smoky flame, and flaming melted fragments continually drop from it in a truly characteristic manner. Ignited in coarse powder in an open crucible it gives off abundant smoke and flame, then seems to boil quickly, and a "coke" is left of the bulk of the original material, and showing when turned out a complete cast of the interior of the crucible. The ash of the "coke" is grey, and consists mainly of silicate of alumina, at least no lime or a mere trace is dissolved by acid, while some alumina is taken up and a great deal of solid remains undissolved. The powdered Oil-Coal digested with benzine and with ether does not more than sensibly color these fluids, but some residue remains on evaporation in each case.

The bituminous coal occurring with the oil-coal had the usual characters belonging to its species; it was black, brilliant, and very brittle. The proximate analyses of the two are placed side by side: and it will at once be obvious that they contrast very strikingly.

	Oil-coal.	Bitum. coal.
Volatile matters,	66·56	33·58
Fixed carbon,	25·23	62·09
Ash,	8·21	4·33
	100·00	100·00

The following is the ultimate analysis of the Oil-Coal, for which I am indebted to Mr. Slessor, assistant to Prof. Anderson of Glasgow, whose aid I requested from want of the necessary apparatus:

Carbon,	80·96
Hydrogen,	10·15
Nitrogen[a] (by loss),	0·68
Ash (as above),	8·21
	100·00

[a] with oxygen and sulphur?

The Oil-Batt appears to be decidedly a shale, and a specimen from Bear Brook, Frazer Mine, gave these results:

Volatile matters,	30·65
Fixed carbon,	10·98
Ash,	58·47=100·00

I proceed to compare the Torbane Hill mineral, and the "Albert coal." A specimen of the former, examined at the time of the trial before mentioned, in 1853, gave me—

Volatile matters,	71·17	Carbon,	66·00
Fixed carbon,	7·65	Hydrogen,	8·58
Ash,	21·18=100·00	Nitrogen,	0·55
		Sulphur,	0·70
		Oxygen,	2·99
		Ash,	21·18=100·00

In a recent examination, a specimen of "Albert coal" gave:

Volatile matters,	54·39	Carbon,*	87·25
Fixed carbon,	45·44	Hydrogen,	9·62
Ash,	0·17=100·00	Nitrogen,	1·75
		Ash,	0·17
		Oxygen and S,	1·21=100·00

These results I place in a table with corresponding data obtained from bituminous and cannel coals, the specific gravities of the substances, and the ratio of carbon to hydrogen as calculated directly from analysis, with the authority for the numbers. The first 7 analyses are from the Report on Coals by Sir H. Delabeche and Dr. Playfair, 1848, and Memoirs Geol. Survey, vol. ii; the 8th and 9th from Miller's Chemistry, iii, p. 201, and the 10th from Report of Trial on Torbane Hill "Coal," Edinburgh, 1853; the 11th has not been heretofore published.

Name or Locality.	Sp. gr.	Proximate analysis.			Ultimate analysis.					Ratio of C : H.	Observers.
		Volatile matters.	Fixed carbon.	Ash.	C.	H.	N.	S.	O.		
Powell's Duffryn.	1·326	15·70	81·04	3·26	83·26	4·66	1·45	1·77	0·60	100 : 4·82	H. How.
† Mynydd Newydd.	1·310	25·20	71·56	3·24	84·72	5·76	1·56	1·21	3·52	100 : 6·79	"
Ebbw Vale.	1·275	22·50	76·00	1·50	89·78	5·15	2·16	1·02	0·39	100 : 5·73	"
Grangemouth.	1·290	43·40	53·08	3·52	79·85	5·28	1·35	1·42	8·58	100 : 6·61	"
‡ Fordel Splint.	1·25	47·97	48·03	4·00	79·58	5·50	1·13	1·46	8·33	100 : 6·93	"
Broomhill.	1·25	40·80	56·13	3·07	81·70	6·17	1·84	2·85	4·37	100 : 7·55	"
§ Parkend Sydney.	1·283	42·20	47·80	10·00	73·52	5·69	2·04	2·27	6·48	100 : 7·73	"
Wigan.	1·276	39·64	57·66	2·70	80·07	5·53	2·12	1·50	8·08	100 : 6·90	Vaux.
¶ Lesmahagow.	1·251	56·70	37·26	6·034	73·44	7·62	?	1·145	?**	100 : 10·43	W. A. Miller.
Capledrae.	?	?	?	25·40	56·70	6·80	1·90	0·35	8·80	100 : 11·99	A. Fyfe.
Torbane Hill, Scot.	1·170	71·17	7·65	21·18	66·00	8·58	0·55	0·70	2·99	100 : 13·00	H. How.
Hillsborough, N. B.	1·091	54·39	45·44	0·17	87·25	9·62	1·75	?	?††	100 : 11·02	J. Slessor and H. How.
Pictou, N. S.	1·1039	66·53	25·23	8·21	80·96	10·15	0·68	?	‡‡	100 : 12·53	J. Slessor and H. How.

In this table we observe in the first place the resemblance of the last three substances in having a density much below that of

* This analysis kindly furnished by Mr. Slessor, as regards C, H, and N.
† Welsh bituminous coals. ‡ Scotch bituminous coals.
§ English bituminous coals. ‖ English cannel. ¶ Scotch cannel coals.
** N and O =11·761 per cent. †† S ? and O =1·21. ‡‡ N, S, and O =0·68.

all the others, and secondly, that in all the bituminous coals but one the volatile matters are considerably less in amount than the fixed carbon, while in the cannel coals this is also the case with one of the two whose proximate analyses are given; as regards the other we see that it contains a large percentage (=11·761) of N and O which would of course be included as "volatile matters," and in the last three substances the volatile matters greatly exceed the fixed carbon. It is well known that in discussions on the chemical nature of coals, etc., much stress is laid on the relative proportions of these products, and also on the ratio of carbon to hydrogen, but it appears to me that an important element in the calculation has generally been omitted, or has not received due attention; I allude to the quantity of oxygen present, which of course can only be found by ultimate analysis. It is constantly stated that the gas and oil-producing value of a coal is indicated by the weight lost in coking, but this is obviously true only to a certain extent, and indeed is in some cases clearly untrue, for if we do not take into consideration the effect of oxygen present we cannot make a just comparison of the chemical nature of the substances, nor find the ratio of C : H, neither can we give the real gas or oil value, when, as above, from eight to ten per cent of what is generally supposed to be carbon and hydrogen is really oxygen with nitrogen. If for example we consider the effect of the oxygen in the composition of the substances given in the table we shall see the last three present such differences from the others as to strengthen the position of those who decline calling them 'coals.' Limiting our view to the cannel-coals, which, as seen above, exhibit the ratio of C : H apparently equal or nearly so to that in the substances in question, we observe that they all contain much more oxygen, and if we deduct the equivalent quantity of H in all, as is theoretically necessary for arriving at the heating power, we shall find this similarity greatly lessened; as thus,

		Ratio of C : H after deducting H=O.
Cannel coal from	Wigan,	100 : 5·65
" "	Lesmahagow,*	100 : 8·71
" "	Capledrae,	100 : 10·05
Mineral "	Torbane Hill,	100 : 12·43
" "	Hillsborough,	100 : 10·85
" "	Fraser Mine,	100 : 12·43

The last three substances should prove, theoretically, the excellent 'Oil-Coals' they are known to be. Of course the practical yield of oil will vary according to the manipulation, the perfection of the manufacturing processes and the quality of samples employed, but the following statement of the comparative

* After allowing two per cent for nitrogen.

amounts of oil afforded by some of the above may be taken as a good illustration of the point brought forward in this paper. I am indebted for these details to H. Poole, Esq.:—

In Scotland the Lesmahagow cannel coal gives 40 gallons crude oil, and 32 gallons rectified oil per ton.

At M'Lellan Brook the Fraser Oil-Coal gives 40 gallons crude oil per ton.

At Coal Brook the Fraser Oil-Coal and Oil-Batt together give 53 gallons per ton.

At M'Cullock Brook the Fraser Oil-Coal gives 77 gallons per ton.

The "Albert coal" gives 100 gallons per ton.

The "Torbane Hill coal" gives 125 gallons per ton.

And some picked samples of Oil-Coal from Fraser Mine, tried in Boston, U. S., gave no less than 199 gallons of oil per ton.

Windsor, Nova Scotia, May, 1860.

ART. XI.—*The Great Auroral Exhibition of Aug.* 28*th to Sept.* 4*th,* 1859*; and the Geographical Distribution of Auroras and Thunder storms.*—5TH ARTICLE. By Prof. ELIAS LOOMIS.

SINCE the publication of our former articles on the great aurora of Aug. 28th to Sept. 4th, we have received some additional observations which we here subjoin.

1. *Extract from a Journal of the weather in Swedish Bothnia,* (lat. 67° N., long. 22° E.), *by* ROBERT RAWLINSON, *copied from the London Times of Oct.* 5.

Aug. 27–28. Morning gloomy; clouds gray and electric looking; a sort of dense "Noah's Ark" sky.

Aug. 28. Night, heavy rain.

Aug. 29. Night bright and clear, but bitterly cold; ice a quarter of an inch thick round tent.

Aug. 30. Day fine; sun very hot. No observation at night.

Aug. 31. Day fine, clear and calm. No observation at night.

Sept. 1. Morning, heavy clouds showing for wet.

Sept. 2. Heavy thunder storm at night, vivid lightning and deluges of rain.

Sept. 3. Morning cloudy; thick mist over forest.

Sept. 4. Morning cloudy; heavy dew in night, thick fog.

Sept. 5. Cloudy, but fine.

2. *Observations at St. Petersburg, Russia,* (lat. 59° 56′), *communicated by* Prof. A. T. KUPFFER, *Director of the Central Physical Observatory.*

From Aug. 28th to Sept. 4th, 1859, the disturbances of the magnetic instruments at St. Petersburg were very remarkable not only for their extent but for their long continuance. The magnetic observations are ordinarily made every hour, but on the preceding days, observations were made every five minutes. We have not room to publish these observations entire, but the following table exhibits the most remarkable deflections of the magnetometers.

The north pole of the unifilar magnetometer moves towards the east, when the numbers of the scale increase. The value of one division of the scale is 26″·3.

When the numbers on the scale of the bifilar magnetometer increase, the magnetic intensity increases. The value of one division of the scale is 0·0001 of the total intensity.

In these observations the day is supposed to begin at noon.

Hour.	Unifilar.	Bifilar.
August 28th.		
h m		
0 0	132·5	185 4
3 0	105·8	193·7
9 0	143·0	207·0
13 0	108·0	186·0
16 0	169·3	420·0
19 0	122·0	420·0
22 0	192·0	420·0
23 05	105·0	106·7
23 20	165·0	230·2
23 40	102·0	127·5
August 29th.		
0 0	135 5	209·0
0 45	103 0	168·0
0 55	194·0	321·2
1 5	111·0	66 5
2 40	143·0	222·8
4 0	132·3	187 0
5 5	220·5	257·5
5 30	140·0	195·0
6 10	185·0	190·0
7 20	100·0	98·0
7 35	90·0	100·0
8 0	150 0	138·0
8 45	129·0	133·3
19 0	153·0	124·0
23 0	137·4	125·0
September 1st.		
0 0	101·2	136·5
2 0	75·5	183·0
9 0	150·8	167·9
12 0	120·0	159·0
h m		
20 0	151·5	419·0
20 30	410·5	419·0
21 0	530·0	419·0
21 10	601·5	419·0
21 30	302·0	216·0
21 40	9·0	12·0
21 45	off scale	7·8
21 55	94·0	0·0
22 5	off scale	119·0
22 15	320·0	177·0
22 30	85·0	0 0
22 50	123·7	98·5
23 15	10·0	150·0
23 30	128·0	100·0
23 45	59·5	86·0
September 2nd.		
0 5	135·0	205·3
0 35	67·0	150·0
0 45	134·5	65·0
1 55	81·0	178·0
2 20	186·0	393·0
2 25	82·0	555·0
2 40	off scale	304·5
3 20	134·0	306·0
3 40	13·5	519·2
4 0	147·0	292·5
4 45	85·0	420·5
5 0	122·4	571·5
5 10	41·0	501·0
5 50	104·5	277·5
6 45	100·0	172·2
8 0	234·5	420·0
8 5	242·0	303·4
h m		
8 25	115·0	166 0
10 35	95·3	90·3
12 0	70·1	50·2
12 15	63·0	10·2
12 25	47·3	off scale
12 50	160·9	70·6
13 35	156·0	off scale
14 45	116·2	78·0
15 15	122·4	44·6
16 20	113 0	108·0
17 5	98·0	104 2
18 35	160·0	109 5
20 20	147·0	129·0
20 55	127·0	123·0
21 45	150·3	115·2
22 5	136·0	106·3
23 55	120·0	136·0
September 3rd.		
0 15	126·0	143·0
0 50	111·9	127·5
1 55	85·0	247·0
2 55	75·5	210·0
4 15	50·0	237·0
4 55	45·0	229·0
6 45	110·0	275·0
8 0	144·0	142·0
8 40	44·0	55·0
9 0	101·0	52·5
10 0	158·0	154·0
13 40	72·0	off scale
14 20	65·0	35·5
16 20	157·0	135·2
18 15	150·0	140·0

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
h *m*						*h* *m*		
20 55	127·0	84 0	September 4th.			5 10	142·5	158 0
21 20	84·2	109 0	*h* *m*			5 40	180 0	247·0
21 45	131·0	109·0	0 40	104·0	122·0	7 20	124·5	127·0
23 55	110·7	140·4	1 15	98·0	135 6	9 35	113.5	150·0
			2 35	113·8	134·0	16 0	140·2	134·2
			3 20	79·0	233·0	20 0	126·3	159 0
			4 0	101·5	257·0	23 0	115·0	136·0

From the preceding observations we see that the range of the magnetometers for each day was as follows:

	Unifilar magnetometer.			Bifilar magnetometer.		
	d	*d*	*d*	*d*	*d*	*d*
Aug. 28	From 102 to 192	= 90	= 39′ 27″	From 106·7 to 420	=313·3	=·031
Aug. 29	" 90 " 220·5	=130·5	= 57 12	" 66 5 " 321·2	=254·7	=·025
Sept. 1	" 0 " 601·5	=601·5	=263 39	" 0 " 419 0	=419·0	=·042
Sept. 2	" 0 " 242	=242	=107 44	" 0 " 571·5	=571·5	=·057
Sept. 3	" 44 " 158	=114	= 49 58	" 0 " 275	=275	=·027
Sept. 4	" 79 " 180	=101	= 44 16	" 122 " 257	=135	=·013

It should be remarked that in the preceding table, 0 indicates that the magnets passed beyond the range of their scales; so that we can only conclude that on Sept. 1st and 2nd the range of the magnets certainly *exceeded* the values here given.

3. *Observations at Catherinenburg, Russia* (lat. 56° 50′, long. 60° 34′ E.), *communicated by* Prof. A. T. Kupffer.

The following observations are arranged in the same manner as those from St. Petersburg. The value of one division of the unifilar scale is 33″·4. The value of one division of the bifilar scale is 0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.	Hour.	Unifilar.	Bifilar.
			h			*h*		
September 1st.			22	off scale	off scale	8	536·5	300·5
h			23	off scale	180·0	10	481·0	295·7
0	496·0	308·5				11	519·0	297 0
2	490·0	311·6	September 2nd.			14	512·0	283·0
6	507·5	319·0	0	434 0	238·0	20	539·0	285·4
12	510·3	321·0	3	519·0	281·2	23	528 0	291·8
18	525 0	320·2	6	427·0	396·0			
21	580·0	201·0	7	520·0	412 0			

4. *Observations at Barnaul, Russia* (lat. 53° 20′, long. 83° 27′ E.), *communicated by* Prof. A. T. Kupffer.

The value of one division of the unifilar scale is 32″·8. One division of the bifilar scale =0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.
	August 28th.	
h		
0	391·5	171·6
1	383·6	175·4
4	392·6	185·0
10	402·3	171·4
14	408·0	161·5
18	423·4	69·0
20	431·8	7·2
23	412·5	off scale

Hour.	Unifilar.	Bifilar.
	August 29th.	
h		
0	416·5	off scale
2	402·5	20 0
3	382·0	121·0
5	420·0	48·0
6	437·0	94·0
9	404·0	116·0
10	422·0	106·5
18	417·5	125·0
23	408·5	107·5

Hour.	Unifilar.	Bifilar.
	September 1st.	
h		
22	413·2	152·5
23	519·3	off scale
	September 2nd.	
0	off scale	466·5
2	358·5	32·3
6	385·0	262·0
8	350·6	156·5
9	440·5	77·4
19	397·0	166·6
23	404·0	107·5

5. *Observations at Nertchinsk, Russia,* (lat. 51° 18′, long. 119° 20′ E.), *communicated by* Prof. A. T. Kupffer.

The value of one division of the unifilar scale is 33″·8. One division of the bifilar scale =0·0001 of the total intensity.

Hour.	Unifilar.	Bifilar.
	September 2nd.	
h		
0	347·0	285·1
1	383·0	235 4
2	635·4	370·1
3	355·5	321·5
4	214 0	181·0
5	294·8	205·6
6	327·1	224·1
7	339·5	256·5
8	335 0	250·4
9	312·2	288·5
10	311 9	243·5
11	450·0	260·0
12	333·0	246·0
13	345·4	243·0
17	327 0	269·0
19	352·0	279·0
23	343·5	243·0

6. *Observations at Athens, Greece* (lat. 37° 58′), *by* J. F. L. Schmidt, *Director of the Observatory.* (Communicated through Rev. Mr. King, Am. Missionary at Athens.)

The aurora was not seen at Athens Aug. 28th and 29th. Both evenings were very clear and still, especially Aug. 28th. Aug. 29th from 7h to 8h P. M. some clouds were seen in the west over the Morea. Aug. 30–31 was likewise clear, with a very few small clouds. Aug. 31st, in the evening, lightning in the N.W. Sept. 1st, evening, partly clear, partly cloudy, with lightning in the west.

Sept. 2d, 7h 15m A. M., beginning of a storm from the west, rain and thunder; at 8h 30m A. M., rain, hail and lightning. From noon to 12h 40m P. M., violent shower from the west. Then became clear with sunshine. The evening was clear, and in the north there appeared a dark bank of ordinary cloud (not the dark segment of the aurora), above which, from 7h 30m P. M. to 8 P. M., was seen a fine aurora of a carmine red color. The cloud bank, which extended 60° in azimuth, was elevated somewhat above the horizon, so that stars were seen beneath it. The centre of the auroral light was not in the north, but N.N.W. On the west it was bounded by Cor Caroli, and on the east by

α Persei. No streamers or fluctuations of light were observed. By 10 P. M. the cloud bank had disappeared, the auroral light having disappeared previously.

Sept. 3d. The entire day was clear; and at 4 P. M. I went on board a steamer for Syra. From 7½–8 P. M., near the island Egina I saw in the north and northwest the carmine red light of an aurora. From 9½–10½ P. M., near Cape Sunium, a faint trace of the aurora was still seen; but no dark segment, streamers or fluctuations of light.

7. *Observations at Camp Simeahmoo, Washington Territory*, (lat. 49°, long. 122° 30′ W.), *by* ARCHIBALD CAMPBELL, *Commissioner of N. W. Boundary Survey.*

At 8 P. M. Aug. 28, 1859, a diffused light, without definite form, was observed a little east of north, covering about one-fourth of the heavens, which gradually increased to the west, sending across from east to west an arch of a whitish color, the arch itself being much brighter than the circumjacent light. This arch remained visible until 2 A. M. At $9^h\ 25^m$ P. M. strongly marked rays became visible, which rising from the horizon converged to a point on the arch a little south of the zenith, and in this position remained visible about one hour. The rays in the northwest were of a pink color, those in the southeast were purple, alternately brightening and fading to a whitish color. At midnight, all disappeared except the arch, and at intervals undulating flashes of light appeared, not visible longer than three seconds. Occasionally streamers shot up from the horizon, the lower part disappearing before the upper part had reached the zenith. Sometimes these streamers were broad at the horizon, and came to a point near the zenith, and sometimes the reverse. The arch before mentioned was easily identified, and was still visible at 2^h A. M., and probably remained so until daylight, which at that season, in this latitude, occurs not long after that time. This arch was situated very little, if any, to the southward of the zenith, and was the limit of all light in that direction. The light was sufficiently intense, between 11 and 12 o'clock, to enable a person to read the ordinary print of a newspaper. After the aurora was fully formed, it remained stationary, and did not move either to the west or east. At midnight, the barometer stood at 30·13; external thermometer 64° F.

August 29th a faint diffused light was seen in the north at 9 P. M., and was still visible at midnight.

August 30th a similar light was first seen at 8½ P. M. and was still visible at midnight. No observations were made after midnight. There was no exhibition of the auroral light at this place from Aug. 31st to Sept. 4th.

The view of the northern horizon at this place is cut off by a dense forest of firs, and the sight of the heavens in that direction is some 5 or 6 degrees above the horizon.

8. *Observations at Hamilton, C. W.* (lat. 43° 16′ N.), *by* Dr. J. HURLBURT.

Answers to the questions in our first article, vol. xxviii, p. 408.

1. On the 28th of August the sky was overcast by a dark sombre cloud-like substance, but which was not cloud as the stars could be seen through it. There was no dark segment resting on the northern horizon, but one was seen at the south between 8 and 9 P. M. rising 8° or 10° in the centre. Sept. 1st between 8 and 9 P. M. this dark segment was well defined at the north, with an altitude of about 10°, and skirted the horizon fully 120°. At 1 A. M. Sept. 2d, an unusually large segment was distinctly defined at the south, where it rose fully 15° in the centre, and stretched over 130° of the horizon.

2. At 8 P. M. Aug. 28th, and from 1 to 3 A. M. Sept. 2d, the streamers of the aurora converged to a point a little east of the meridian, and 15° or 20° south of the zenith, forming a brilliant corona.

3. Aug. 28th, both in the southwest and southeast there was a dark red spot about 14° in breadth, and extending from altitude 35° to alt. 55°. Both spots presented the same appearance, and hung in corresponding parts of the heavens east and west.

4. At 1 A. M. Sept. 2d, the whole of the southern half of the sky was lighted up, resembling the sky at late dawn.

5. On the night of Aug. 28th, the most frequent and conspicuous color was red and its different shades. The aurora of Sept. 1st was scarcely at all marked with any of these colors. The light was chiefly white, resembling the dawn just before sunrise.

6. The aurora of Aug. 28th was most brilliant at 8 P. M. It was also very brilliant at 1 and 3 A. M. Aug. 29th. The aurora of Sept. 2d was most brilliant at 1 A. M.

9. *Observations at Rome, N. Y.* (lat. 43° 13′), *by* Mr. EDWARD HUNTINGTON.

During the auroral display on the evening of Sept. 2d, there was a very rapid and incessant flashing of white light, like waves running up to the corona, the light being intermitted for some seconds after the passage of each wave, and the next wave pursuing the same course, and *following certain curvatures which remained constant for a considerable time*, perhaps half an hour. There was a succession of waves flashing up from a point in the northwest, where the effect was as if a luminous fluid were in its course passing through a narrow strait, from which it issued,

expanding like water violently forced through a narrow passage out upon a broad level area, and being at the same time somewhat deflected from its former course. Fig. 1 is designed to convey some idea of this appearance.

1.

2.

From another point nearly in the east, there was a succession of waves of light, directed to a point north of the corona, but turning suddenly into it nearly at a right angle with their former course, the light disappearing after the passage of each wave, and being renewed after a short interval in precisely the same form. Fig. 2 is designed to give some idea of this appearance.

10. *Observations at Cleveland, Ohio* (lat. 41° 31′), *by* Capt. B. A. Stanard.

Aug. 28th, at $8^h\ 30^m$ P. M., the aurora began to show itself, lighting up the northern sky, rising towards the zenith, in a broad belt of luminous haze of about 40° in width, the southern edge starting a little to the westward of the star Arcturus, and running through the head of Hercules, a little south of Altair in Aquila, through the head and neck of Pegasus. The eastern end was bright enough to light up the edges of the detached masses of cumuli that were driving over from the north. In the zenith and the western end it was of moderate brightness.

At 9 P. M. another belt began to rise up in the north, and as the convex edge attained a height of about 40° it began to shoot out long, attenuated, bright rays, close together, moving slowly to the westward, and reaching to the zenith. Near the convex edge they were of a bright yellow, changing as they shot up to

orange, and near the zenith to a bright red, the middle and lower ends remaining yellow and orange. As the fiery points of the rays shot into the broad belt overhead, which had still remained like a belt of luminous haze or fog, the whole thing was changed in an instant into a bright red color, deepening as it neared the eastern horizon, to a bright crimson, and at the western end, near the star Arcturus, into a bright scarlet, gradually growing fainter in the zenith, and increasing in brightness nearer the horizon.

At 9^h 15^m P. M. it resolved itself into converging rays. The zenith at that time was covered by a massive cloud, covering the point where the rays would meet, which must have been near the star β Cygni.

At 9^h 30^m P. M. the whole had disappeared, except a steady white light in the north, very bright, forming a curve from the western edge of Böotes, through Cepheus and Perseus. The northern horizon was too clouded the whole time to see anything near it.

Sept. 2nd, at 7^h 45^m P. M., the northern lights began to show, forming a low narrow arch extending from the N. W. to N. E., the lower edge about 10° in height, well defined, of moderate brightness, and remaining without perceptible change until 8^h 45^m P. M., when bright vertical rays shot up simultaneously the whole length of the arch, of an exceedingly bright white color, with no perceptible motion E. and W. Shooting slowly up towards the zenith, at an altitude of about 45°, they began to change color rapidly, varying from white, yellow, orange, and at the upper ends red; diffusing a soft pink color over the northern sky at an altitude of about 45°. At 9 P. M. they gradually disappeared, the arch was broken up, leaving some irregular white blotches in the north, which faded away and disappeared altogether. Then commenced a series of quick, sudden flashes of undefined light; here and there in the north, scimetars bearing a strong resemblance to heat lightning, sometimes in undefined rays, and sometimes in undefined shimmering light.

This continued until 9^h 45^m P. M., when a double arch was formed of two narrow belts of light about 15° in height, running from Canes Venatici to the southern edge of Perseus, the bright star Capella shining through the narrow black space between the two arches.

At 9^h 55^m P. M. bright rays suddenly shot up in quick successive flashes from the lower through the upper arch, reaching nearly to the zenith, and moving slowly to west until they reached the constellation Corona Borealis, lighting up the north western sky with yellow, orange and red. After the last rays from the east had passed the Pole, there commenced a sudden flashing of horizontal wavy bands from the upper arch towards the zenith.

At $10^h\ 10^m$ P. M. the rays and arches disappeared and the northern sky seemed to be covered with a steady white light, with horizontal wavy bands of dark haze rolling up in quick succession, and vanishing as they attained an altitude of about 60°, continuing until $10^h\ 30^m$ P. M. and gradually fading away.

11. *Observations at Fort Bridger, Utah Ter.* (lat. 41° 14′, long. 110° 33′), *by* KIRTLEY RYLAND, *Assist. Surgeon U. S. Army.*

Sept. 1st, 1859, a brilliant aurora was seen at this place. It was first observed about 11 P. M. and attained its greatest brilliancy before midnight. It extended from the northeastern horizon to the southern horizon, and was in fact a Borealo-Austral Aurora. Generally the light assumed the form of spikes and bars, but high above the horizon in the northeastern sky was a large blotch or spot, whose diameter was perhaps three times that of the constellation Orion. This blotch was of a deep crimson color, and remained for a considerable time unchanged in form, color or intensity, and faded gradually away. In the other portions of the aurora the light appeared to spout from the horizon, in the shapes already named, frequently reaching the zenith, and was of great brilliancy. It appeared to flow gradually from N.N.E. to the southern sky.

12. *Observations made at Cantonment Burgwin, New Mexico* (lat. 36° 21′, long. 105° 42′), *by* W. W. ANDERSON, *Assist. Surgeon U. S. Army.*

A member of the guard mounted Sept. 1st observed a light reflected from the clouds on that night about 10 P. M. as he was walking post, the clouds having probably just then thinned out or broken away a little. No other member of the guards from Aug. 28th to Sept. 5th saw anything unusual during the intervening nights. By reference to the Meteorological Register I find that the weather was cloudy during the whole time that the aurora was visible elsewhere. There was rain on the last four days of August, and also on the 1st, 2nd and 4th of September.

The exhibition was witnessed at Taos, about ten miles north of us, by persons residing there, but was not observed with sufficient attention to enable them to answer any of your questions with accuracy. A physician, Dr. Ferris, who arrived at Taos from Pike's Peak during the fall, states that he was in the South Park at the time, and thinks that it was on the night of the 28th of August that the Aurora was observed by him. He saw it but one night. Streamers were seen to converge to the zenith about 10 P. M., but the aurora was not seen in the southern half of the heavens. The light was like a large fire in the distance, so that at first it was thought to be an extensive fire on the

mountains. The aurora exhibited sudden flashes, and there were pulsations like waves of light rushing up from the horizon. I have been told that Capt. John G. Walker, of the Rifle Regiment, wrote a description of the aurora as it appeared at Fort Defiance (lat. 35° 44′), where it is said to have been very brililant.

13. *Observations at the Sandwich Islands* (lat. 20° N., long. 157° W.), *from the Pacific Commercial Advertiser.*

The Advertiser of Sept. 8th, 1859, states: "There was quite a display of the Aurora a few nights since, visible at Honolulu. Broad fiery streaks shot up into and played among the heavens, almost as beautifully as those which are sometimes seen in more northern climes."

The Advertiser of Sept. 17th contains the following letter from S. E. Bishop, dated Lahaina, Sept. 9th. "Your statement that the Aurora was seen in Honolulu enabled me to account for the phenomenon I observed here a few nights since. At 10 P. M. I noticed a bright, unsteady crimson glow upon the sky, extending from N. E. to N., and about 35° of altitude. It resembled the reflection of a great conflagration at twenty or thirty miles distance, and I attributed it to heavy fires on the other side of the mountain. I was puzzled however by the fact that the clouds which rested on the mountain did not give the slightest reflection of the supposed fire. Moreover the light was far too pure and rich a crimson to have been caused by a fire.

14. *Observations at Porto Rico, West Indies* (lat. 18° N.), *by* M. du COLOMBIER, *from L'Institut of Feb. 1st*, 1860.

Having awakened at $2^h\ 30^m$ A. M., Sept. 2nd, I was greatly astonished to see my windows, which looked towards the north, brightly illumined by a brilliant purple light. Rising immediately, I perceived that this light proceeded from a magnificent aurora, which, according to the testimony of the guard, commenced at 2^h A. M. and was observed till 4^h A. M. The luminous rays, red, purple and violet, extended even to the zenith. The oldest inhabitants of the place declared that they had never before seen a phenomenon of this kind.

15. *Observations at Santiago de Chili* (lat. 33° 26′ S.), *by* C. MOESTA, *Director of the Observatory.*

The aurora you allude to did occur at this and several other places in the south of Chili, during the night between Sept. 1st and Sept. 2nd, 1859. I did not witness the phenomenon myself, but it appears that the aurora was visible from about half past 1 until 4 A. M., showing a motion to the west. The watchmen were much alarmed at the colored light with which the southern

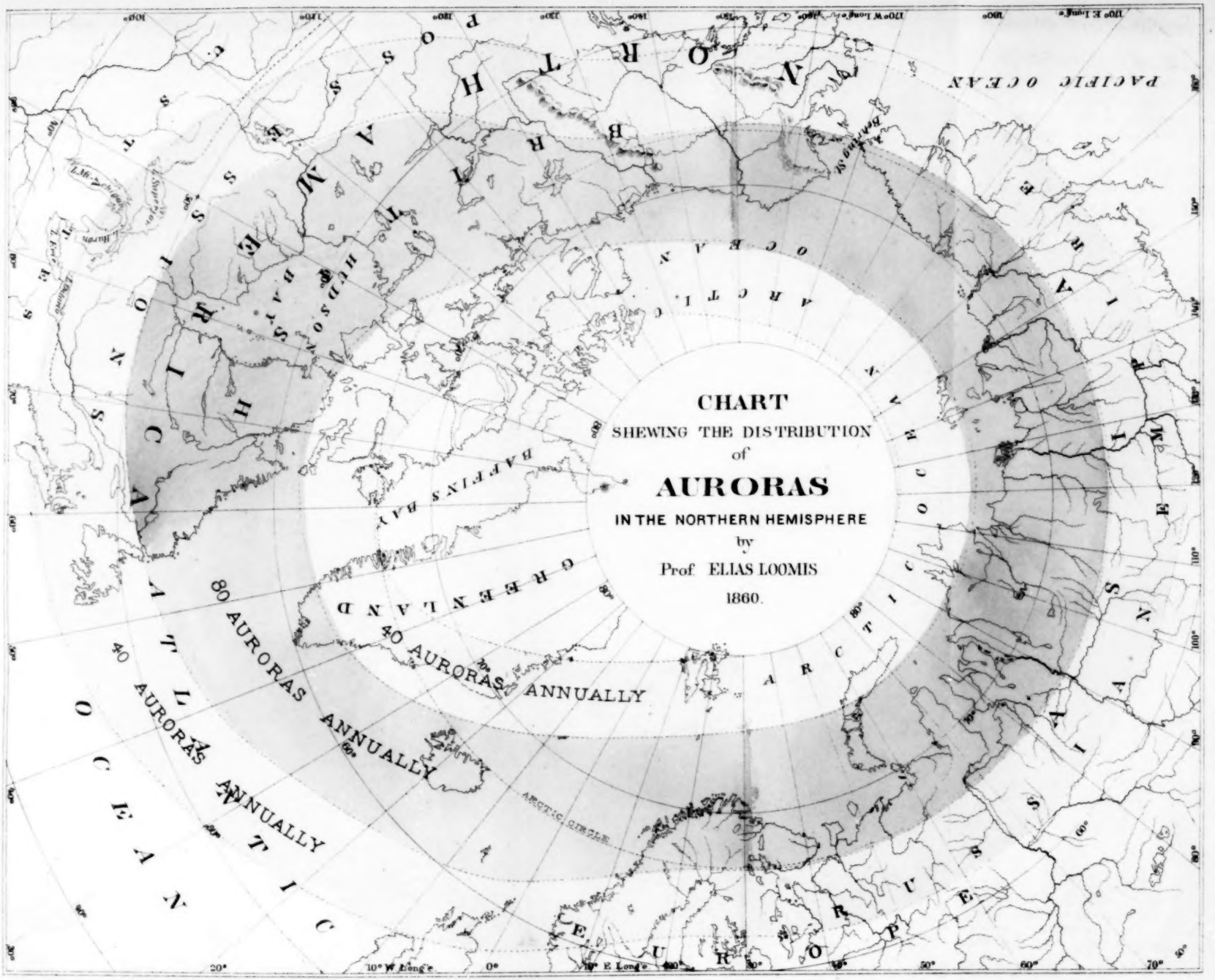


CHART
SHEWING THE DISTRIBUTION
of
AURORAS
IN THE NORTHERN HEMISPHERE
by
Prof. ELIAS LOOMIS
1860.
80 AURORAS ANNUALLY
40 AURORAS ANNUALLY
40 AURORAS ANNUALLY
ARCTIC CIRCLE
ATLANTIC OCEAN
PACIFIC OCEAN
ARCTIC OCEAN
NORTH AMERICA
EUROPE
GREENLAND
BAFFINS BAY
HUDSONS BAY
BERINGS S.
20°
10° W Long.e
0°
10° E Long.e
30°
50°
60°
70°

part of the sky was covered, which gave rise to the belief that a small village about three leagues south of Santiago was on fire. This seems to be the first time that a polar light has been seen at Santiago. No notice has reached me as to its appearance north of this place.

16. *Observations near Cape Horn* (lat. 57° S., long. 66° W.), *by* RICHARD SCHUMACHER, *communicated by C. Moesta.*

Mr. Richard Schumacher, assistant to the Chili Observatory, was at the time on board a ship near Cape Horn. Being informed Aug. 29th that an aurora had been seen during the preceding night, he begged the mate of the vessel would let him know if there should be another. Accordingly he was awakened during the night from Sept. 1st to 2nd, between 2 and 4 A. M., when the aurora was already in its splendor. In a southerly direction there appeared a bright yellowish light forming an ellipse, whose diameters were as two to one, the centre of the ellipse being elevated about 15° above the horizon. The part of the horizon below this light seemed to be a cloudy mass of a dirty reddish color. From this ellipse, emanated a red light, apparently all over the heavens up to the zenith, and thence onwards to the north. Mr. S. did not distinguish any beams or columns of light, though the sky seemed illuminated all round nearly uniformly, except that the light of the ellipse was much brighter than the surrounding parts. There were also some light transparent clouds discernible near the zenith. The light was so brilliant that he could easily read the title page of the Nautical Almanac, and distinguish the seconds hand of a box chronometer.

The vessel was at noon, Sept. 1st, in lat. 57° 8′, long. 66° 38′ W.
" Sept. 2nd, " 57 36 " 66 47 "

ON THE GEOGRAPHICAL DISTRIBUTION OF AURORAS IN THE NORTHERN HEMISPHERE.

Auroras are very unequally distributed over the earth's surface. They occur most frequently in the higher latitudes, and are almost unknown within the tropics. At Havana (lat. 23° 9′) but six auroras have been recorded within a hundred years; and south of Havana, auroras are still more unfrequent. As we travel northward from Cuba, auroras increase in frequency and brilliancy; they rise higher in the heavens, and oftener attain the zenith. The following tables furnish the most precise data I have been able to collect for constructing an auroral chart of the northern hemisphere. Column first gives the name of the station of observation; columns second and third its latitude and longitude; column fourth the average number of au-

roras observed per year; column fifth, the greatest number of auroras recorded in a single month; column sixth shows the number of years embraced in the comparison; and column seventh shows the authority for the statement.

The numbers for several of the stations are derived from an article contained in vol. viii of the Smithsonian Contributions, entitled, Record of auroral phenomena observed in the higher northern latitudes, compiled by Peter Force. Such observations are indicated by the word 'Force' in the last column.

TABLE I.—*Average annual number of Auroras in North America and its vicinity, from longitude 30° to 170° west from Greenwich.*

Place.	Latitude.	Longitude.	Per year	Richest month.	Years embraced.	Authority.
	° ′	° ′				
Havana,	23 9	82 22	1/16	1	100	Am. Jour., [2], xxviii, 403.
San Francisco,	37 47	122 26	½	1	4	Smithson. Report, 1854, p. 258.
Sacramento, Cal.,	38 34	121 27	½	1	9	Am. Jour., [2], xxix, 260.
Washington, D. C.,	38 53	77 0	9	3	4	Gilliss's Met. Observations.
Wilmington, Del.,	39 44	75 32	8	4	7	Am. Jour., xxxiii, 299.
Philadelphia,	39 58	75 10	6	3	4	Bache's Met. Observations.
New York,	40 42	74 0	14	5	1	N. Y. Regents Rep., 1850, 255.
New Haven,	41 18	72 55	26	12	16	E. C. Herrick's observations.
North Salem, N. Y.,	41 26	73 38	8	2	10	N. Y. Regents Rep., 1850, 258.
Deerfield, Mass.,	42 33	72 35	4	2	1	Am. Jour., iv, 337.
Fayetteville, Vt.,	42 58	72 40	21	8	11	Am. Jour., vols. xii to xxiv.
London, C. W.,	42 58	81 25	27	7	4	{ Regents Rep., 1850, 290–293. { Am. Jour., [2], xiv, 156.
Toronto, C. W.,	43 39	79 23	39	9	12	" " "
Kingston, C. W.,	44 8	76 40	36	11	4	Am. Jour., [2], xiv, 156.
Somerville, N. Y.,	44 26	75 20	70	14	2	" " "
Halifax, N. S.,	44 39	63 36	55	13	2	" " "
Montreal, C. E.,	45 31	73 32	34	10	2	Regents Rep., 1850, 290 & 291.
Quebec, C. E.,	46 49	71 12	42	12	4	" " "
St. John's, N. F'nd,	47 33	52 38	52	10	3	" " "
Michipicoton,	47 56	85 2	43	9	2	Am. Jour., [2], xiv, 156.
Matawagomingen,			26	7	1	" " "
Moose Factory,	51 10	81 0	141	19	1	" " "
Martin's Falls,	51 52	86 45	79	14	1	" " "
Cumberland House,	53 56	102 16	104	25	2	Gehler's Wörterbuch, vii, 144.
Athabasca Lake,	58 43	111 18	91	21	2	{ Athabasca Obs., p. 145. { Am. Jour., [2], xiv, 156.
Frances Lake,	61 30	129 0	33		2	Lake Athabasca, p. 148.
Lewis and Pelly,	61 30	130 0	36	12	1	Am. Jour., [2], xiv, 156.
Fort Simpson,	61 51	121 32	50	24	1½	Athabasca Obs. & Am J., xiv, 156.
Great Slave Lake,	62 46	109 1	105	28	2	Capt Back, 1833–1835.
Godthaab,	64 10	51 53	72	15	6	Observat. Meteorolog., 166–228.
Fort Enterprise,	64 28	113 6	142	28	1	Force, pp. 24–35.
Fort Norman,	64 40	124 45	32	15	½	Force, p. 54.
Fort Franklin,	65 12	123 12	43	17	½	Force, pp. 60–64.
Youcon,	66 0	147 0	24	7	½	Am. Jour., [2], xiv, 156.
Winter Island,	66 11	83 10	25	8	1	Force, pp. 66–72.
Fort Hope,	66 32	86 56	39	13	1	Force, pp. 75–77.
Fort Confidence,	66 54	118 49	100	30	2	{ Athabasca Obs., 324–350. { Am. Jour., [2], xiv, 156.
Peel's River,	67 27	134 30	65	16	1	Am. Jour., [2], xiv, 156.
Jakobshaven,	69 12	51 0	18	5	8	Observat. Meteorolog., 82–164.

TABLE I.—*Continued.*

Place.	Latitude.	Longitude.	Per year	Richest month.	Years embraced	Authority.
	° ′	° ′				
Felix Harbor,	69 59	92 1	23	10	1	Force, pp. 84–87.
Point Barrow,	71 21	156 15	131	26	1	Phil. Trans., 1857, p. 497.
Port Bowen,	73 14	88 55	47	15	1	Force, p. 91.
Melville Island,	74 47	110 49	26	8	1	Force, pp. 97–103.
Smith Sound,	78 37	70 40	10	5	2	Dr. Kane's Obs.

The following auroral observations are generally confined to brief periods of time, and have therefore less importance than the preceding. They are nevertheless of some value in deciding in what part of the world auroras are most frequent and brilliant. These notices are all taken from Force's article in the Smithsonian Contributions, vol. viii.

TABLE II.—*Notices of Auroras in North America and its vicinity, from longitude 30° to 170° west from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.
	° ′	° ′	
Cedar Lake,	53 13	100 10	Extremely brilliant and covered the whole sky.
Off Cape Farewell,	56 17	42 51	Frequently most brilliant.
At sea,	57	49	Occurred almost every night.
York Fort,	57 2	93	Very few winter nights without the aurora. One may read distinctly by it.
At sea,	57 30	45	Whole southern hemisphere illumined. Gave nearly as much light as the full moon.
At sea,	58 12	49 15	Radii shot from the southward.
At sea,	58 30	44 30	Far surpassed anything of the kind observed at Port Bowen.
At sea,	59	50	Seen in every part of the heavens.
At sea,	59 58	59 53	Brilliant coruscations.
Hoarak,	59 59	44 36	Four luminous arches.
At sea,	60	56	Whole sky illuminated.
Kikkertak,	60 4	43 2	Unusually brilliant coruscations.
Nennortalik, Greenland,	60 8	45 16	Of every day occurrence. Brighter than the full moon.
Davis Straits,	60 10	49 40	Yellow and reddish coruscations, extending near the zenith.
At sea,	61 4	49 50	Illumined the whole southern sky.
Davis Straits,	61 37	52	The whole sky was one living fire of aurora.
Davis Straits,	61 58	54 40	Most brilliant aurora danced to the zenith.
At sea,	62 30	63	Aurora seen in the south.
Rankin's Inlet,	62 35	93	Aurora very bright, spreading all over the sky.
Hudson Strait,	62 45	72 24	Aurora unusually splendid.
Good Hope,	64 10	51 42	Auroras always spring up in the E. or S.E.
Cape Lavenorn,	64 30	39 30	Auroras unfrequent.
At sea,	65	63	Very brilliant, spread all over the heavens.
Southampton Island,	65 28	84 40	Visible during the whole of the night.
At sea,	65 50	61	Very brilliant; shooting rays to the zenith.
Hudson's Bay,	66 11	82 53	Very brilliant.
Chamisso Island,	66 13	161 47	Very brilliant.
At sea,	66 30	59	Seen in the south.
Kotzebue's Sound,	66 30	163	Aurora always seen to the northward.
Fort Macpherson,	67	135	Six auroras seen in fifteen days.

TABLE II.—*Continued.*

Place.	Latitude.	Longitude.	Auroras.
	° ′	° ′	
Cape Krusenstern,	67 8	163 46	Shot up to the zenith; pink, purple, and green rays.
Fort Good Hope,	67 28	130 54	Spread all over the sky.
Behring's Sea,	68 30	167	Unusually brilliant display.
Hearne's Sea,	68 48	115 31	Most superb display.
Igloolik,	69 15	81 45	2 auroras in Nov.; 1 in Dec. 1822. Generally faint.
Baffin's Bay,	70 43	63 44	Faint aurora to southward.
Baffin's Bay,	71 20	62 28	Brilliant aurora.
Baffin's Bay,	72 10	68 36	Eleven auroras seen in Feb. 1851.
Somerset House,	72 48	95 41	Seldom seen in 1833.
Baffin's Bay,	72 49	70 59	Nine auroras seen in Jan. 1851.
Batty Bay,	73 17	91	Great luminous rays issued from the zenith.
Austin's winter quarters,	74 10	94 16	11 auroras in Jan. 1851; 12 in Feb. 1851; and 4 in March, 1851.
Lancaster Sound,	74 18	82 10	Eleven auroras in Dec. 1850.
Griffith Island,	74 30	95 20	Vastly inferior to those seen in more southern latitudes.
At sea,	74 31	111 38	Aurora faint.
Off Beechy Island,	74 40	92	Ten auroras seen in Nov, 1850.
Assistance Harbor,	74 40	94 16	Much less vivid than in more southern latitudes.
Barrow Strait,	74 45	94	Two auroras in Sept. 1850.
Wellington Channel,	74 54	93	Seven auroras seen in Oct. 1850.
Northumberland Sound,	76 52	97	Five auroras seen in Dec. 1852.

The following table exhibits the average annual number of auroras in Europe. The table is arranged in the same manner as Table I.

TABLE III.—*Average annual number of Auroras in Europe.*

Place.	Latitude.	Longitude.	Per year	Richest month.	Years embraced.	Authority.
	° ′	° ′				
Bologna,	44 32	11 23 E.	4	6	24	Mairan Aurore Boreale, p. 505.
Paris,	48 50	2 20 E.	19		64	Gehler Wört., vii, 1, 135.
Montmorency,	49 0	2 19 E.	5	4	27	Cotte Meteorologie, 355.
Carlsruhe,	49 1	8 25 E.	8	5	11	Gehler Wört., vii, 1, 146.
Plymouth,	50 22	4 9 W.	6	5	21	Am. Journal, xxxiii, 298.
Leyden,	52 9	4 29 E.	26		29	Cotte Meteorologie, 355.
Berlin,	52 30	13 23 E.	4	5	28	Mairan Aurore Boreale, 500.
Franecker,	53 12	5 32 E.	25	8	7	Cotte Meteorologie, 355.
Kendal,	54 19	2 45 W.	31	10	7	Dalton's Met. Essays, 54–58.
Makerstoun,	55 35	2 31 W.	31		6	Am. Jour., [2], xi, 139.
Dunse,	55 47	2 20 W.	24	11	10	Phil. Trans. Abstracts, vi, 291.
Upsala,	59 52	17 38 E.	37	14	21	De la Rive Elec., iii, 301.
Christiana,	59 54	10 43 E.	33	13	16	De la Rive Elec., iii, 300.
St. Petersburgh,	59 56	30 18 E.	21	8	11	Mairan Aurore Boreale, 510.
Bossekop,	69 58	23 34 E.	143		1	Pouillet Physique, ii, 663.

The following observations are less definite than the preceding, and are therefore given in a separate table.

TABLE IV.—*Notices of Auroras in Europe and its vicinity from Longitude 60° East to 30° West from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.	Authority.
	° ′	° ′		
Teneriffe,	28 16	16 39 W.	Aurora seen Nov. 1837 for the first time in the memory of man.	Alfred Diston.
Shetland Islands,	60 20	1 9 W.	Constant attendant of clear evenings.	Rees' Cyc., v. iii.
At sea,	60 30	25 W.	A bright arch with coruscations.	Force, p. 13.
At sea,	61	25 W.	Passed through the zenith and eclipsed the moon in splendor.	Force, p. 14.
Iceland,	64	22 W.	Seen almost every clear night.	Henderson, p. 148.
Torneo,	65 52	24 13 E.	Frequently occupy the whole sky. Commonly seen in the north.	Maupertuis, ii, 155.

The following are the most definite observations I have been able to collect from the Asiatic continent.

TABLE V.—*Auroras in Asia and its vicinity from Longitude 60° East to 170° West from Greenwich.*

Place.	Latitude.	Longitude.	Auroras.	Authority.
	° ′	° ′		
Irkutsk,	52 20	103 50 E.	Two auroras seen in Dec. 1735.	Gmelin, p. 434.
Tomsk,	56 35	86 30 E.	Only one aurora per month in 1741.	Gmelin, p. 477.
Catherinenberg,	56 50	63 35 E.	Four auroras seen in 1854.	Kupffer Obs.
Turinsk,	57 45	63 45 E.	Three auroras seen in March 1742.	Gmelin, p. 326.
Kirenskoi Ostrog,	58 0	108 E.	Five auroras seen in March 1739.	Gmelin, p. 458.
Tobolsk,	58 12	68 18 E.	Not much more frequent than under the same latitude in Europe.	Erman, i, 394.
Jeniseisk,	58 30	92 E.	Three auroras seen in Feb. 1738.	Gmelin, p. 453.
Beresov,	63 56	65 4 E.	Sometimes seen for months together throughout the night.	Erman, i, 470.
Virchni Koorina,	66	152 E.	Constant and very brilliant.	Billings, p. 57.
Koliutchin Island,	67 26	175 35 W.	More frequent and brilliant than at Nijnei Kolymsk.	Von Wrangell, 506.
Nijnei Kolymsk,	68 32	160 56 E.	Seen almost every evening.	Von Wrangell, 83.
On the ice,	69 58	168 41 E.	An aurora of extraordinary beauty.	Von Wrangell, 103.
On the ice,	70 20	174 13 E.	Beautiful aurora all night.	Von Wrangell, 318.

If we project all the preceding observations upon a chart, we shall discover considerable uniformity in the distribution of auroras over the earth's surface. If we travel from the equator northward along the meridian of Washington, we find on an

average near the parallel of 40°, only ten auroras annually. Near the parallel of 42°, the average number is 20 annually; near 45°, the number is 40; and near the parallel of 50° it amounts to 80 annually. Between this point and the parallel of 62°, auroras are seen almost every night. They appear high in the heavens, and as often to the south as the north. Further north they are seldom seen except in the south, and from this point they diminish in frequency and brilliancy as we advance towards the pole. Beyond lat. 62° the average number of auroras is reduced to 40 annually. Beyond lat. 67° it is further reduced to 20, and near lat. 78° to 10 annually. If we make a like comparison for the meridian of St. Petersburg, we shall find a similar result, except that the auroral region is situated further northward than it is in America; the region of 80 auroras annually being found between the parallels of 66° and 75°.

Upon the accompanying chart, the deep red color indicates the region where the average number of auroras annually amounts to at least 80; and the pale red color indicates the region where the average number of auroras annually amounts to at least 40. We thus see that the region of greatest auroral action is a zone of an oval form surrounding the north pole, and whose central line crosses the meridian of Washington in lat. 56°, and the meridian of St. Petersburg in lat. 71°. Accordingly auroras are more frequent in the United States than they are in the same latitudes of Europe. On the parallel of 45°, we find in North America an average of 40 auroras annually; but in Europe less than ten.

Geographical Distribution of Thunder Storms.

The geographical distribution of auroras is believed to be intimately related to the geographical distribution of thunder storms. I have therefore made a considerable collection of facts showing the average annual number of days of lightning at different points of the earth's surface. In the following table, column 4th shows the average annual number of days of lightning for the places named in column 1st. Column 5th shows the number of years embraced in the comparison; and column 6th shows the authority for the statements.

Table VI.—*Average number of days of Lightning annually.*

Place.	Latitude	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Georgetown, Guiana,	6 49	58 11 W.	60	11	Brit.Guiana Met.Obs.1846-56.
Ethiopia,	11	36 E.	82	5	Astron. Nachricht., 1204, 62.
Lima, Peru,	12 0	77 2 W.	0		Arago Met. Essays, p. 109.
Abyssinia,	13	37 E.	38	1	Arago Met. Essays, p. 129.
Madras,	13 4	80 16 E.	144	5	Madras Obs., 1841-45.
Central Africa,	12 to 18	4E.to 4 W.	65	2	Barth's Central Africa.

TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Martinique,	14 40	61 3 W.	39		Arago Met. Essays, p. 129.
St. Helena,	15 55	5 54 E.	0	3½	St. Helena Obs., 1840-43.
Guadeloupe,	16 12	61 45 W.	37		Arago Met. Essays, p. 129.
Bombay,	18 50	72 50 E.	42	5	Bombay obs., 1845-49.
Calcutta,	22 35	88 25 E.	60	1	Arago Met. Essays, p. 128.
Rio Janeiro,	23 0 S.	43 14 W.	50·6	6	" " " " "
Patna, India,	25 31	85 10 E.	53	1	" " " " "
Cairo,	30 6	31 26 E.	3·5	2	" " " " 134.
Buenos Ayres,	34 30 S.	58 28 W.	22·5	7	" " " " 130.
Gibraltar,	36 7	5 21 W.	6		Berghaus Annalen, 200, 6.
Athens,	37 58	23 44 E.	11	3	Arago Met. Essays, p. 133.
Palermo,	38 7	13 19 E.	13·5		Kämtz Meteorology, p. 362.
Smyrna, Asia,	38 23	27 11 E.	19	1	Arago Met. Essays, p. 130.
Washington,	38 53	77 0 W.	13·5	3	Gilliss's Met. Observations.
Maryland,	39 0	75 30 W.	41	1	Phil. Trans., 1759, p. 58.
Janina, Turkey,	39 47	20 59 E.	45	10	Kämtz Met., ii, 463.
Pekin, China,	39 53	116 40 E.	5·8	6	Arago Met. Essays, p. 134.
Philadelphia,	39 58	75 10 W.	10	3	Journal Franklin Institute.
New York,	40 42	74 0 W.	25·4	10	N. Y. Regents Rep., 1843-56.
Hudson, Ohio,	41 14	81 25 W.	13	7	Manuscript observations.
North Salem, N. Y.	41 20	73 38 W.	22·7	10	N. Y. Regents Rep., 1843-51.
Litchfield, Conn.,	41 45	73 8 W.	25·0	3	" " " 1851-52.
Amenia, N. Y.,	41 48	73 36 W.	18	1	" " " 1850, 241.
Rome, Italy,	41 54	12 25 E.	42·4	11	Kämtz, Met., ii, 463.
Rodez, France,	42 0	2 39 E.	11	1	Annuaire Met., 1851, p. 122.
Albany, N. Y.,	42 40	73 44 W.	21·6	7	N. Y. Regents Rep., 1840-49.
Marseilles, Fr.,	43 18	5 22 E.	9·3	9	Kämtz Met., ii, 463.
Toulouse, Fr.,	43 36	1 26 E.	17·1	8	Annuaire Met., 1851, 131. Arago Met. Essays, p. 131.
Toronto, C. W.,	43 39	79 21 W.	20	2	N. Y. Regents Rep., 1850, 371.
Lowville, N. Y.,	43 47	75 33 W.	12·7	3	N. Y. Regents Rep., 1840-43.
Newbury, Vt.,	44 6	72 5 W.	8·7	3	" " " 1854-56.
Orange, France,	44 7	5 50 E.	12·2	31	Annuaire Met., 1851, p. 167.
Somerville, N. Y.,	44 10	75 25 W.	17·3	3	N. Y. Regents Rep., 1850-52.
Padua, Italy,	45 24	11 52 E.	41·9	12	Kämtz Met., ii, 463.
La Rochelle, Fr.,	46 10	1 9 W.	21·0	8	Kämtz Met., ii, 455.
Geneva, Switz.,	46 12	6 10 E.	19	4	Gehler, 4, 2, p. 1585.
Bourg, France,	46 12	6 15 E.	37	1	Annuaire Met., 1851, p. 188.
Morges, Switz.,	46 31	6 28 E.	22·8	5	Bibliotheque Universelle, March 1860, p. 229.
Quebec, C. E.,	46 49	71 12 W.	23·3		Arago Met., p. 129.
Dijon, France,	47 19	5 2 E.	16	1	Annuaire Met., 1851, p. 84.
Viviers, France,	47 30	4 40 E.	24·7	10	Arago Met., p. 129.
Ofen, Hungary,	47 30	19 2 E.	28·0	11	Kämtz Met., ii, 459.
Tegernsee, Bav.,	47 30	11 32 E.	23·2	9	" " " 457.
Peissenberg, Bav.,	47 48	11 1 E.	23·0	12	" " " "
St. Andex, Germ.,	47 58	11 12 E.	27·1	12	" " " "
Denainvilliers, Fr.,	48 0	2 20 E.	20·6	24	Arago Met., p. 130.
Munich, Bav.,	48 8	11 35 E.	22·7	12	Kämtz Met., ii, 457.
Vienna, Austria,	48 12	16 20 E.	8·3	20	" " " "
Augsburg, Bav.,	48 21	10 54 E.	22·3	12	" " " "
Tubingen, Würt.,	48 32	9 2 E.	14·6	9	Arago Met., p. 132.
Lugan, Russia,	48 35	39 21 E.	35	1	Berghaus Annalen, 200, 5.
Strasbourg, Fr.,	48 36	7 46 E.	17	20	Arago Met., p. 131.
Giengen, Würt.,	48 37	10 15 E.	21·9	12	Kämtz Met., ii, 456.
Hohenfurth, Boh.,	48 37	17 40 E.	28		Berghaus Annalen, 200, 10.
Stuttgart, Würt.,	48 46	9 10 E.	20·6		Kämtz Met., ii, 456.

TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Versailles, France,	48 48	2 7 E.	15	1	Annuaire Met., 1851, p. 55.
Paris, France,	48 50	2 20 E.	13·6	51	Arago Met., p. 132.
Goersdorff, France,	48 57	7 48 E.	27	1	Annuaire Met., 1851, p. 181.
Regensberg, Bav.,	49 1	11 56 E.	16·9	10	Kämtz Met., ii, 457.
Rehberg, Boh.,	49 6	13 27 E.	29		Berghaus Annalen, 200, 10.
Metz, France,	49 7	6 10 E.	16	1	Annuaire Met., 1851, p. 109.
Tabor, Bohemia,	49 24	16 59 E.	5		Berghaus Annalen, 200, 10.
Rouen, France,	49 26	1 5 E.	13	1	Annuaire Met., 1851, p. 149.
Mannheim, Bav.,	49 28	8 27 E.	20·8	12	Kämtz Met., ii, 456.
Seelau, Boh.,	49 32	17 33 E.	12		Berghaus Annalen, 200, 10.
Brzesnitz, Boh.,	49 34	9 17 E.	27		" " " "
Deutschbrod, Boh.,	49 36	17 55 E.	16		" " " "
Würzburg, Bav.,	49 46	9 54 E.	13·9	8	Kämtz Met., ii, 427.
Brzezina, Poland,	49 49	15 58 E.	27		Berghaus Annalen, 200, 10.
Landskron, Boh.,	49 55	18 57 E.	11		" " " "
Tepel, Bohemia,	49 58	12 52 E.	11		" " " "
La Chapelle, Fr.,	50 0	3 35 E.	15·7	18	Arago Met., p. 131.
Eger, Bohemia,	50 5	14 42 E.	15		Berghaus Annalen, 200, 10.
Prague, Bohemia,	50 6	14 23 E.	17·7	10	Kämtz Met., ii, 457.
Smetschna, Boh.,	50 11	16 22 E.	11		Berghaus Annalen, 200, 10.
Königgrätz, Boh.,	50 13	18 10 E.	28		" " " "
Polpero, Eng.,	50 20	4 10 W.	10	23	Arago Met., p. 133.
Oberwiesenthal, Boh.	50 25	12 58 E.	15		Berghaus Annalen, 200, 10.
Hohenelbe, Boh.,	50 38	17 54 E.	20		" " " "
Altenberg, Boh.,	50 45	13 43 E.	15		" " " "
Tetschen, Boh.,	50 47	16 32 E.	17		" " " "
Brussels, Belg.,	50 50	4 30 E.	16·1	8	Kämtz Met., ii, 455.
Maestricht, Hol.,	50 51	5 42 E.	16·5	11	Arago Met., p. 131.
Zittau, Sax.,	50 54	14 48 E.	19		Berghaus Annalen, 200, 10.
Freiberg, Sax.,	50 55	13 20 E.	13		" " " "
Erfurt, Prussia,	50 58	11 2 E.	14·1	8	Kämtz Met., ii, 457.
Schluckenau, Boh.,	51 1	14 27 E.	14		Berghaus Annalen, 200, 10.
Dresden, Sax.,	51 3	13 44 E.	20		" " " "
Nertschinsk, Rus.,	51 18	119 20 E.	3·1	6	Kämtz Met., ii, 459.
Middleburg, Belg.,	51 28	3 37 E.	21·3	6½	" " " 455.
London, Eng.,	51 30	0 7 W.	8·3	13	Arago Met., p. 133.
Sagan, Prussia,	51 37	15 19 E.	29·3	12	Kämtz Met., ii, 457.
Münster, Pr.,	51 58	7 38 E.	29·7		Berghaus Annalen, 200, 5.
Utrecht, Hol.,	52 6	5 8 E.	15		Arago Met., p. 132.
Leyden, Hol.,	52 9	4 30 E.	13·5	29	" " " 133.
Irkutsk, Rus.,	52 17	104 17 E.	8·5	2	Kämtz Met., ii, 459.
Minden, Pr.,	52 18	8 55 E.	5·8		Berghaus Annalen, 200, 5.
Berlin, Prussia,	52 31	13 21 E.	17·3	120	Kämtz Met., ii, 457.
Lüneberg, Han.,	53 14	10 28 E.	20·2	20	" " " 456.
Barnaul, Russia,	53 20	83 27 E.	24	1	Berghaus Annalen, 200, 6.
Hamburg,	53 33	9 58 E.	10·7		Kämtz Met., ii, 456.
Cuxhaven, Germ.,	53 53	8 44 E.	11·5	10	" " " "
Braunsberg, Pr.,	54 20	19 54 E.	30·7		Berghaus Annalen, 200, 5.
Apenrade, Den.,	55 3	9 25 E.	19		" " " "
Tilsit, Prussia,	55 5	21 45 E.	12·6		" " " "
Slatoust, Russia,	55 11	59 45 E.	14	2	" " " "
Copenhagen, Den.,	55 41	12 35 E.	1	4	Gehler Wört., v. 4, 2, p. 1585.
Memel, Prussia,	55 42	21 6 E.	4	26	Berghaus Annalen, 200, 5.
Moscow, Russia,	55 47	37 44 E.	22·4	9	Kämtz Met., ii, 459.
Kasan, Russia,	55 52	49 30 E.	9	1	" " " "
Jekaterinenburg, Rus.	56 50	60 34 E.	23·3	3	Berghaus Annalen, 200, 5.

TABLE VI.—*Continued.*

Place.	Latitude.	Longitude.	No. per year.	Y'rs embraced.	Authority.
	° ′	° ′			
Skara, Sweden,	58 22	12 28 E.	9·2	25	Kämtz Met., ii, 461.
Stockholm, Sweden,	59 21	18 4 E.	9·3	10	" " " "
Spydberga,	59 38	9 E.	7·7	3	" " " "
Bogoslowsk, Rus.,	59 45	59 59 E.	2	1	Berghaus Annalen, 200, 6.
Petersburg, Rus.,	59 52	30 25 E.	12·4	10	Kämtz Met., ii, 459; Arago.
Abo, Russia,	60 15	22 10 E.	10	12	" " " "
Bergen, Nor.,	60 24	5 18 E.	5·8	7	" " " 461.
Sondmör, Nor.,	62 30	6 20 E.	3·9	12	" " " "
Archangel, Rus.,	64 32	40 33 E.	6·5	18	Berghaus Annalen, 200, 5.
Reikiavik, Iceland,	64 8	22 0 W.	½	2	Arago Met., p. 111.
Fort Franklin,	65 12	123 12 W.	1	2	" " " "
Baffin's Bay,	65 30	80 W.	1	1	" " " 110.
Melville Island,	74 47	110 49 W.	0	2	" " " "
At sea,	75	100 W.	0	1	" " " "

It is obvious from an examination of the preceding table, that thunder storms prevail most frequently in the equatorial regions and diminish as we proceed towards the poles. It is also evident that the frequency of thunder showers depends upon other circumstances than simply latitude; but without stopping to enquire what these circumstances are, we will take the average of the observations included between different parallels of latitude. We thus find that:—

between lat. 0° and lat. 30°	the average number of thunder storms annually is	51·6
" "30 " " 50		19·9
" "50 " " 60		14·9
" "60 " " 70		4·0
beyond "70		0·0

Maury's storm and rain charts of the Atlantic Ocean furnish most important information on the same subject. The following table presents a summary of the results of these charts. The ocean is divided into squares by parallels of latitude drawn at intervals of five degrees from each other, and meridians of longitude at intervals of five degrees. Each square of the following table contains three numbers. The first shows the number of days of observation within the given square; the second shows the number of days of lightning reported, and the third is the number of days of lightning which would occur in one year, as computed from the numbers actually observed. Thus in the square included between the parallels of 30 and 35 degrees of north latitude, and between the meridians of 65° and 70°, the first number is 548, which shows that 1644 observations have been obtained in that square. Each observation represents a period of 8 hours, so that 1644 observations represent 548 days. During this period, lightning was reported on 44 days, which is at the rate of 29 cases for one year.

TABLE VII.—*Days of Lightning in the Atlantic Ocean, by Maury's charts.*

Lat.	80°	75°	70°	65°	60°	55°	50°	45°	40°	35°	30°	25°	20°	15°	10°	5° W.	0°	5° E.	10°	15°	20°
60°	—	—	—	—	—	—	—	—	—	—	—	20	34	41	39	26	10	—	—	—	—
												0	1	0	1	0	0				
												0	11	0	9	0	0				
55	—	—	—	—	—	—	—	50	140	170	231	283	311	420	464	194	—	—	—	—	
								0	2	1	6	6	2	5	4	4					
								0	5	2	9	8	2	4	3	7					
50	—	—	—	—	42	96	306	414	523	579	542	513	493	437	30-	104	—	—	—	—	
					1	0	5	5	7	11	8	11	5	5	2	0					
					9	0	6	4	5	7	5	8	4	4	2	0					
45	—	607	1083	824	893	806	621	527	373	244	132	90	56	43	22		—	—	—	—	
		11	48	56	46	37	24	23	14	5	5	0	0	0	0						
		7	16	25	19	17	14	16	14	8	14	0	0	0	0						
40	81	1398	991	599	464	370	248	160	116	81	114	89	101	113	111	75	—	—	—	—	
	1	70	81	40	24	22	12	3	1	0	7	3	0	1	0	0					
	5	18	29	24	19	22	18	7	3	0	22	12	0	3	0	0					
35	511	755	548	255	241	287	329	298	249	131	58	43	78	26	1		—	—	—	—	
	41	42	44	7	17	8	11	13	17	2	0	1	1	0	0						
	29	20	29	10	26	10	12	16	25	5	0	8	5	0	0						
30	648	464	379	316	131	117	188	242	319	221	70	51	29				—	—	—	—	
	33	15	24	23	12	7	6	4	29	6	2	0	0								
	19	12	23	27	33	22	12	6	33	10	10	0	0								
25	105	120	87	217	212	89	87	151	268	221	113	45	5	—	—	—	—	—	—	—	
	5	6	1	8	2	1	0	2	8	2	0	0	0								
	17	17	4	14	4	4	0	4	11	3	0	0	0								
20	81	107	51	61	153	180	109	101	150	237	213	53	2	—	—	—	—	—	—	—	
	2	9	2	5	1	3	3	3	0	5	0	1	0								
	9	31	14	30	2	6	10	11	0	8	0	7	0								
15	22	0	18	18	32	129	198	169	138	222	278	207	75	—	—	—	—	—	—	—	
	0	0	0	0	0	4	4	18	9	13	2	3	3								
	0	0	0	0	0	11	7	39	24	21	3	5	15								
10	—	—	—	—	—	—	96	223	211	246	556	336	161	239	27	23	18	—	—	—	
							1	6	16	14	42	12	1	6	0	0	0				
							4	10	28	21	27	13	2	9	0	0	0				
5 N.	—	—	—	—	—	—	32	140	204	335	668	421	112	36	121	78	33	74	—	—	
							0	8	4	13	40	32	6	3	3	9	1	3			
0	—	—	—	—	—	—	0	21	7	14	22	28	19	30	9	42	11	15	—	—	

| | | |

We see in this table abundant evidence that the frequency of lightning depends upon other circumstances than simply latitude. Throughout the western half of the Atlantic Ocean, lightning occurs *three times* as frequently as throughout the eastern half of that ocean. If we take the average of all the observations upon the same parallel of latitude, we shall find the number of days of lightning to be as follows:

	North Atlantic.	South Atlantic.
From lat. 0 to lat. 5°	20	6
" " 5 " 10	17	2
" " 10 " 15	14	1
" " 15 " 20	8	2
" " 20 " 25	7	6
" " 25 " 30	19	9
" " 30 " 35	19	7
" " 35 " 40	19	6
" " 40 " 45	15	9
" " 45 " 50	5	8
" " 50 " 55	5	3
" " 55 " 60	4	4

The average frequency of lightning in the North Atlantic is *two and a half times* as great as in the South Atlantic; also the average frequency of lightning on land, at least in the northern hemisphere, appears to be *more than twice* as great as in the North Atlantic. The bearing of the preceding facts upon the theory of auroras will be considered hereafter.

ART. XII.—*On the Products of the Distillation of Common Rosin;* by JAMES SCHIEL, of St. Louis.

THE chemical process taking place in the distillation of common rosin or colophony, and the two different liquids which are obtained by that distillation, have as yet escaped the attention of chemists; the whole process is almost completely wrapped in darkness. The two liquids just mentioned are distinguished as *essence of rosin* (in Europe "German oil of turpentine") and *rosin oil:* the former is thin, of a light yellow color, and a strong, almost aromatic odor; the latter is a thick liquid of a somewhat disagreeable odor.

The *essence of rosin* is a mixture of two substances, which may be separated by fractional distillation if often repeated. During this distillation the liquid passing over sometimes assumes a milky aspect, produced by the formation of a small portion of water and a trace of acetic acid; a small piece of lime or baryta instantly renders the liquid perfectly transparent. Of the two liquids into which the essence of rosin separates, the first is very thin, perfectly colorless and transparent, and of a strong, refracting power. I call it *colophonon.* The specific gravity of

colophonon is 0·84 at 14° C., its boiling point 97° C. (Barom. ·756); with concentrated sulphuric acid it forms a brown liquid from which water separates a green oil, having much the smell of Ol. anthos. Chlorhydric acid produces a similar transformation. With potassium it gives rise to a lively emission of gas and coagulates into a brown-yellow mass. Heated in a closed vessel above the boiling point it colors brown and assumes an odor of peppermint.

The composition of colophonon is expressed by the formula

$$C_{11}H_{18}O_2,$$

One hundred parts consist of

	Calculated.	Found.		
Carbon,	79·52	78·90	78·95	79·50
Hydrogen,	11·85	11·59	11·61	12·01
Oxygen,	9·63	9·51	9·44	9·49
	100·00	100·00	100·00	100·00

The determination of the specific gravity of (vapor of) colophonon gave the number 5·1, which however I consider merely as an approximation. It is remarkable, that this substance, whose composition is distinguished from that of *phoron* by C_2H_4 should have a boiling point as low.

The second constituent of the essence of rosin has its boiling point at 160° C., and possesses all the chemical properties of oil of turpentine. The analysis of it showed it to be composed of

Carbon,	87·44
Hydrogen,	11·78

The formula of oil of turpentine $C_{10}H_{16}$ requires

Carbon,	88·23
Hydrogen,	11·77
	100·00

As it does not seem to have any effect on the plane of polarization, it may be identified with the *therebène* of Deville.

The oil of rosin, with a boiling point above that of mercury, does not seem to have a constant composition; at least, there is sometimes a difference between the raw oil and the refined oil. The raw oil, which has the property of fluorescence, loses this property in a great degree by a very simple refining process. This process consists simply in placing the earthen vessels containing the raw oil on the hot wall around the neck of the iron retort, from which the rosin is distilled, and allowing them to stand there for some hours, keeping them covered up. A portion of the raw oil that had been heated in the water-bath in contact with caustic lime showed the composition $C_{20}H_{28}O_2$ of the *resineine* of Deville and Frémy, viz:

	Calculated.	Found.
Carbon,	84·51	84·70
Hydrogen,	9·80	9·69
Oxygen,	5·69	5·61
	100·00	100·00

A portion of the refined oil, treated in the same way, gave the formula $C_{15}H_{20}O_2$, viz:

Carbon,	86·44	86·25
Hydrogen,	9·62	9·95
Oxygen,	2·94	2·77
	100·00	100·00

Further investigation will show whether this difference is purely accidental, and there is good reason to believe it to be so; or whether there is really a difference in the composition of the two products.

The gases formed during the distillation of the rosin burn with a bright light and may be used for illumination. I have analyzed a portion of these gases, collected in glass tubes which were sealed up with the blowpipe, in the gas-room of Prof. Bunsen. They contain 14·96 p. c. carbonic acid, 11·45 p. c. of oxyd of carbon, 5·89 p. c. ethylene and butylene (ditetryl), besides oxygenated nitrogen very nearly in the proportion of 1 : 6. Towards the close of the distillation the oxygen nearly disappeared, and a portion of light carburetted hydrogen made its appearance.

I have to remark that it must be left undecided whether there may not be an admixture of some propylene with the above 5·89 parts of ethylen and ditetryl, as we do not yet possess any means of analyzing these homologous gases. The analysis of the three homologous gases, ethylene, propylene and butylene, would furnish the equations:

$$\begin{aligned} x+\ \ y+\ z&=V\\ 2x+\ 3y+4z&=B\\ 2x+2{\cdot}5y+3z&=C \end{aligned}$$

V being the volume analyzed, B the volume of carbonic acid formed by the combustion, and C the contraction. From the first of these equations we have $x=V-z-y$, and this value of x introduced into the two other equations, makes them

$$\begin{aligned} y+2z&=B-2V\\ 2y+4z&=C-2V \end{aligned}$$

The second of these is evidently the half of the first and $C=\frac{1}{2}B$. For the determination of y and z we therefore have only *one* equation. If there is a large quantity of the three gases to be disposed of, they may be absorbed by sulphuric acid and the alcohols of the two first may be distilled off after dilution with water.

By sending a current of common coal-gas slowly for an entire week through a number of flasks containing sulphuric acid, then diluting with water and distilling; I found on the surface of the water distilled over, a light oily substance, collecting in large drops, and having a strong, disagreeable odor. It is very likely that to this substance dissolved in the odorless parts of the gas is due the offensive odor of coal-gas. I hope yet to obtain enough of this oily substance for an analysis.

Heidelberg, May 1, 1860

ART. XIII.—(1.) *An account of the fall of Meteoric Stones at New Concord, Ohio, May 1st*, 1860; by Prof. E. B. ANDREWS, of Marietta College. With (2.) *Computations respecting the Meteor;* by Prof. E. W. EVANS, of the same Institution. *To which are added further notices of the same by* D. W. JOHNSON, Esq. and Dr. J. LAWRENCE SMITH.

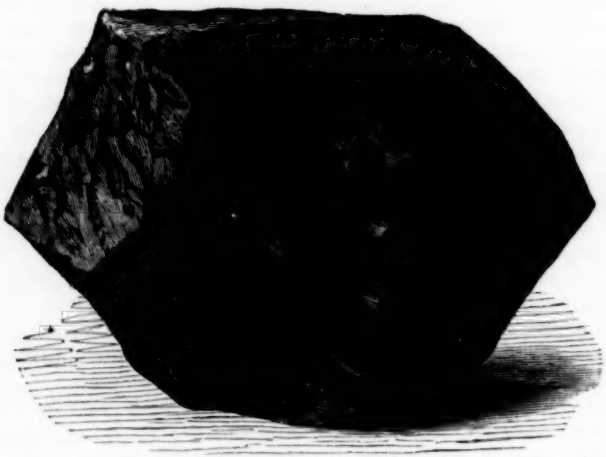
(1.)

ABOUT fifteen minutes before one o'clock on the first day of May, 1860, the people of southeastern Ohio and northwestern Virginia were startled by a loud noise, which was variously attributed to the firing of a heavy cannon, to the explosion of steamboat boilers, to an earthquake, and to the explosion of a meteor. In many cases houses were jarred. To persons within doors the noise generally seemed as if produced by the falling of a heavy soft body upon the chamber floor. Many persons heard a rumbling reverberation which continued for a few seconds. The area over which this explosion was heard was probably not less than one hundred and fifty miles in diameter. At Marietta, O., the sound came from a point north or a little east of north. The direction of the sound varied with the locality. An examination of all the different directions leads to the conclusion that the central point, from which the sound emanated, was near the southern part of Noble county, Ohio.

At New Concord, Muskingum Co., where the meteoric stones fell, and in the immediate neighborhood, there were many distinct and loud reports heard. At New Concord there was first heard in the sky, a little southeast of the zenith, a loud detonation, which was compared to that of a cannon fired at the distance of half a mile. After an interval of ten seconds another similar report. After two or three seconds another, and so on with diminishing intervals. Twenty-three distinct detonations were heard, after which the sounds became blended together and were compared to the rattling fire of an awkward squad of soldiers, and by others to the roar of a railway train. These sounds, with their reverberations, are thought to have continued for two minutes. The last sounds seemed to come from a point in the southeast 45° below the zenith. The result of this cannonading was the falling of a large number of stony meteorites upon an area of about ten miles long by three wide. The sky was cloudy, but some of the stones were seen first as "black specks," then as "black birds," and finally falling to the ground. A few were picked up within twenty or thirty minutes. The warmest was no warmer than if it had lain on the ground exposed to the sun's rays. They penetrated the earth from two to three feet. The largest stone, which weighed 103 lbs., struck the earth at the foot of a large oak tree, and after cutting off two roots, one five inches in diameter, and grazing a third root,

it descended two feet ten inches into hard clay. This stone was found resting under a root which was not cut off. This would seemingly imply that it entered the earth obliquely. It is said that other stones which fell in soft ground entered the earth at a similar angle. They must have been flying in a northwest direction. This fact, added to the other facts, that the detonations heard at New Concord came lower and lower from the zenith toward the southeast, and that the area upon which the stones fell extends with its longer axis in a southeast and northwest direction, would imply that the orbit of the meteor, of which these stones are fragments, extended from southeast to northwest. This conclusion is confirmed by the many witnesses who saw, at the time, a luminous body moving in the same direction. It is a fact of some interest that the larger stones were carried by the orbital force further than the small ones, and were found scattered upon the northwest end of the area referred to. This fact is readily explained by the larger proportional surface presented to the atmospheric resistance in the smaller stones. The stones thus far found vary in weight from a few ounces to over a hundred pounds. They show a decided family resemblance. All are coated with a black crust and show a bluish gray feldspathic interior with numerous brilliant points of nickeliferous iron. Although in some instances the edges remain quite sharply

1.



defined, generally they show that they have been rounded by fusion. The accompanying figure shows the appearance of the larger stone now in the cabinet of Marietta College. Viewed from most positions this stone is angular and appears to have

been recently broken from a larger body. On one side it is much rounder and smoother, and this (the outer surface in the figure) appears to be a part of the original surface of the main meteor. Two of its edges extend more than a foot in length, and two of its diameters are fourteen inches. In the small stones the edges are, I think, more rounded than in the larger ones. The angle at A, fig. 2, is an exact copy of a specimen in my possession. The dotted line shows the thickness of the crust.

2.

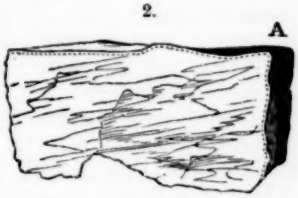
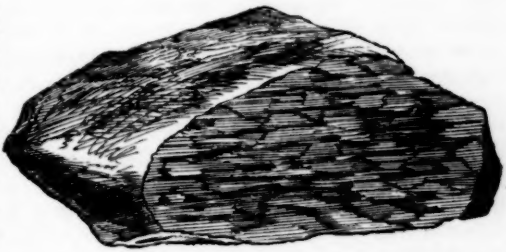


Figure 3 represents the appearance of a small stone, one side of which shows a surface only partially glazed. There was evidently a flaw in this little meteorite, and the heat entering the crack was only sufficient to fuse the surface in a very slight degree. The heat apparently penetrated the crack in straight lines as if driven backward by the high velocity. The edge of the stone surrounding this peculiar surface is a feather edge made by the melting of the metallic crust in an unusual manner.

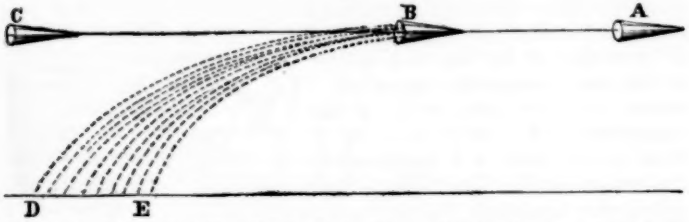
3.



In the examination of this interesting meteoric phenomenon, I am led to believe that the people of New Concord and in the immediate vicinity of the district where the stones fell, heard different sounds, and consequently of different origin, from those heard by people living at a greater distance. The former heard many distinct detonations followed by a rumbling roar like that of thunder. The latter heard but a single explosion followed by a somewhat similar rumbling noise but less distinct. This explosion seemed to take place at a point in the air over the southern part of Noble County. The people of the northern part of the same county heard it in a southern or southeastern direction, and *not in a northwestern direction towards New Concord.* This fact would indicate that the great explosion which was heard more than seventy-five miles away, took place in Noble County, and that the several distinct detonations heard at and near New Concord were directly connected with the falling of the several stones in that district. A diagram will illustrate this and also one or two other points.

By the careful computations of Prof. Evans the meteor passed in its orbit from the point A over Newport, Washington Co.,

4.



Ohio, to the point C, which is quite as far to the northwest as New Concord represented on the diagram by D. The south-eastern limit of the meteoric shower is shown by E. B is the point over Noble County where the explosion was almost universally *heard* to take place. If it took place there, then the onward orbital velocity would take the stones forward from twenty-five to thirty miles before they would drop to the earth between D and E. It is evident that the stones were thrown off from the meteor before it reached the point C, as the stones could hardly have been thrown backward towards E. It is also to be inferred that the meteor passed onward beyond C, although the clouds prevented further observations in that direction. The detonations heard between D and E must be accounted for by the rapid passing of the several stones through the lower atmosphere.

(2.) *Prof. Evans's Computations.*

Owing to the cloudy state of the atmosphere, the time was unfavorable for observing such facts as are necessary for the accurate determination of the height of the meteor, the direction of its path, its size and its velocity. After careful investigation, however, the following results have been obtained.

1. *Direction of its path.*—The district along which the meteorites are known to have fallen is about ten miles long and from two to three miles wide, extending in a northwesterly direction from a little west of the village of Point Pleasant, in Guernsey County, to within a mile of New Concord, in Muskingum Co.* The fragments fell with a northwesterly inclination. This is proved both by the testimony of those who saw them descend, and by the direction in which they were subsequently found to have penetrated the earth. As the sky along this district was overcast with clouds, the main body of the meteor was not seen

* As nearly as can be made out from the data, the path of the meteor appears to have crossed the Ohio River in lat. 39° 30′, long. 81° 20′, and to have disappeared in lat. 40° 2′, long. 81° 90′.—Eds.

by those who witnessed the fall of the fragments; but the sounds, as heard by them, first proceeded from the zenith, and gradually receded towards the southeast. This seemingly contradictory fact agrees perfectly with the hypothesis that the course of the meteor was northwesterly; for if it approached with a velocity greatly exceeding the velocity of sound, the explosions which occurred last must have been the first heard. At some distant stations towards the south and west the view was not wholly obstructed by clouds; and there are many witnesses who relate that a few minutes before any report was heard, they saw a large ball of fire pass across the northern sky, towards the northwest. By tracing out the line along which the reports were loudest and seemed to proceed from the zenith, I am led to the conclusion that the meteor passed over the eastern end of Washington County, then across the interior of Noble County, then over the southwestern corner of Guernsey and the northeastern corner of Muskingum, with a direction of about 42° west of north.

2. *Its height above the earth.*—Mr. William C. Welles of Parkersburg, Virginia (lat. 39° 10′, long. 81° 24′), a gentleman of liberal education, testifies that being about three miles east of that place at the time of the occurrence, he happened to look up to the northeast of him, and saw a meteor of great size and brilliancy, emerging from behind one cloud and disappearing behind another. When about 35° east of north he thinks its altitude was 65°. Now the distance, in a direction 35° east of north, from his station to the line directly under the meteor's path, is 20 miles. Calculating from these data I find for the vertical height, taken to the nearest unit, 43 miles. This was at a point in Washington County near the border of Noble.

Mr. C. Hackley testifies that he saw the meteor from Berlin in Jackson County. It crossed a cloudless space in the northeast, and he thinks its altitude, at the highest point, was 30°. Now the distance from Berlin to the nearest point under the meteor's path is 70 miles. These data give nearly 41 miles for its vertical height over Noble County, a few miles to the south of Sarahsville (lat. 39° 53′, long. 81° 40′).

Many other reliable witnesses have been found who saw the meteor through openings in the clouds from various points west of its path; and whose testimony so far agrees with the foregoing as to give results ranging between 37 and 44 miles. Care has been taken as far as possible to verify the data in each case by personal examination of the witnesses. The angles have in most instances been taken as pointed out by them from their respective posts of observation. It is unfortunate that no case has come to our knowledge in which the meteor was seen from the region east of its path. But it was a circumstance in some

respects favorable to the definiteness of the observations made from the west side, that the observers in nearly all cases saw the meteor only at one point, or within a very small space on the heavens. It is impossible to reconcile the various accounts without granting that its path was very nearly as above described, and that its height did not vary far from 40 miles as it crossed Noble County.

In regard to the time which intervened, at different places, between seeing the fire-ball and hearing the report, the statements are so vague that not much reliance has been placed upon them. It may be remarked, however, that they will essentially agree with the foregoing conclusions, if we suppose that the loudest explosion took place in the southern part of Noble County.

I will add under this head the statement of Mr. Joel Richardson, of Warren, Washington County, who from a place six miles west of Marietta, saw the meteor as much as 15° or 20° west of north, at an altitude of about 45°. The direction in this case was so oblique to the meteor's path, that the data are of little value for simply determining the height; but they are important on account of their connection with the place of the meteor's last appearance. Mr. Richardson was visited by the writer, and his testimony was subjected to close scrutiny. If we take the azimuth at 15° west of north, we shall have a distance of 41 miles to the line under the meteor's path; and these data will give about 41 miles for its vertical height over a point not more than a mile from New Concord, at the extreme western limit of the district along which the meteorites were scattered. If we take the azimuth at 20° west of north, both the distance and the height will be greatly augmented. I have found two persons living near Bear Creek, nine miles north ot Marietta, who make statements closely corroborating that of Mr. Richardson.

3. *Velocity of the meteor.*—Mr. Welles furnishes data from which we can now determine approximately the meteor's rate of motion. As this gentleman is somewhat accustomed to astronomical observation, his judgment as to angles may be strongly relied upon. He thinks he saw the meteor pass from a point 50° east of north to a point 20° east of north in about three seconds. These two points in the meteor's path are over the townships of Newport in Washington County, and Elk in Noble County. The distance between them is 12 miles. According to these data, then, its relative velocity was about four miles a second. No other statement regarding the velocity has been obtained that is sufficiently definite to be of any value.

4. *Its size and shape.*—Those who saw the fire-ball from stations not less than 20 and not more than 30 miles to the westward, agree in stating that it appeared as large and as round as

the full moon. Its intense brilliancy may have produced exaggerated conceptions of its size. But if we take the minimum apparent diameter of the moon, and the minimum distance of the meteor (its height being assumed as 40 miles) we shall have for its diameter 38-hundredths, or about three-eighths of a mile.

The train is described as a cone, having its base upon the fireball. As seen from near Parkersburg its length was estimated at twelve times the diameter of the ball. The part next the base appeared as a white flame; but not so bright as to render the outline of the ball indistinct. About half way toward the apex it faded into a steel blue.

(3.) *Notice of the Fall from* D. M. JOHNSON, Esq., *of Coshocton, O.*

[Mr. Johnson's notice of this shower of meteoric stones is the result of a visit to the locality a few days after the event. We copy the following particulars from Mr. Johnson's account as giving circumstantial detail and historic interest to the facts recounted by Profs. Andrews and Evans. Mr. Johnson also adds a chemical analysis of the meteor.—EDS.]

Two carpenters, Samuel L. Hines and Samuel M. Noble, were at work near the house on the farm of Jonas Amspoker, of New Concord. Upon hearing the first report they looked up and saw two dark looking objects, apparently about the size of an apple, come through a cloud, producing a twirling motion in the vapor of the cloud. One of them they saw fall to the ground about one hundred and fifty yards from where they stood. The other one passed behind the house out of their sight. They went immediately to the one which they saw strike the ground, and found it at the bottom of a hole two feet deep. When taken out it was still warm and in a few seconds dried the moist earth adhering to its surface. It was found to weigh 51 pounds.

Nathanael Hines, who was ploughing in a field adjoining Mr. Amspoker's place, heard a report like the blasting of rocks in a well, followed by several smaller reports. He looked up and saw a black body descending to the earth at an angle of about 30° to the vertical. It struck the ground about two hundred yards from him. Repairing to the place he found that in its descent it came in contact with the corner of a fence, breaking off the ends of the three lower rails, and entering the ground about eighteen inches. It was warm, and had a sulphurous smell. This stone was not weighed, but it is estimated to have been between 40 and 50 pounds in weight before any portions were broken off from it. This was probably the stone that the carpenters saw but lost sight of when it passed behind the house.

James M. Reasner was in his house at the time of the explosion, but hearing a noise like striking against the door with the fist he went out, when his attention was attracted by a whizzing

sound over head. Looking up he saw what appeared to be a black streak descending in a slanting direction towards the earth. After he heard that stones had fallen in that vicinity he sought for and found a stone weighing 36½ pounds.

Wm. Law was in his house, one mile east of Concord. Upon hearing the first report he went out into the yard. He heard a buzzing sound passing over the house in a northwest direction, and saw the sheep running in a field not far from the house. Hearing that stones had fallen he went to the field in which the sheep were and found a stone weighing 53 pounds. It had fallen upon the end of a partially decayed log, through which it passed and buried itself it the ground. This was the largest stone that had been found at the time I visited the district. But I since learn that the one described by Prof. Andrews was found after my visit to the place.

A *blazing* meteor was seen from other parts of the state on the same day. The Columbus Statesman of May 5th says that "near McConnellsville several boys observed a huge stone descend to the earth, which they averred looked like a red ball, leaving a line of smoke in its wake." McConnellsville is 25 miles south of Concord.

Mr. D. Mackley of Jackson Co., in a communication to the Cincinnati Commercial, says, "On the first day of May, at precisely half past 12 o'clock, I was standing on the platform at the railroad station in Berlin when I saw, in a northeast direction, a ball of fire, about 30° above the horizon. It was flying in a northerly direction with great velocity. It appeared as white as melted iron, and left a bright streak of fire behind it which soon faded into a white vapor. This remained more than a minute, when it became crooked and disappeared." Berlin is about 80 miles southwest of Concord.

The meteor seen from McConnellsville and Berlin was undoubtedly the same that exploded and fell in Guernsey County. No one of the many persons who saw the stones fall and were in the immediate vicinity at the time, noticed anything of the luminous appearance described by those who saw it from a distance.

All the stones that I have yet seen have the same general appearance. They are irregular blocks, and are covered with a very thin black crust, which looks as if it had been fused. The edges of the blocks are not sharp but rounded, and the faces present the usual pitted appearance of meteorites. They absorb water with a hissing sound. The fragments are of a bluish gray color. Under the lens five substances can be detected. A snow-white mineral is largely disseminated throughout the mass. A clearer white mineral can be distinguished in some specimens. Metallic grains are quite numerous, a yellowish brown mineral

in patches, and black particles scattered over the surface. One specimen had very thin veins of a shining black mineral. When in large masses the stone is exceedingly tough, requiring repeated blows with a hammer to fracture it, but when broken into small pieces it can be crushed with ease in an agate mortar.

The specific gravity of the mass was found to be 3·5417. On analysis one gramme of the stone was found to contain:—

Silica,	·51250
Protoxyd of iron,	·25204
Magnesia,	·08873
Alumina,	·05325
Lime,	·00785
Iron,	·08803
Nickel,	·02360
Sulphur,	·01184
Chromium,	*trace.*
Phosphorus,	*trace.*
Water,	·00035
	1·03819

Coshocton, Ohio, June 4th, 1860.

(4.) Prof. J. LAWRENCE SMITH, M.D., of *Louisville, Ky.*, informs us that, on hearing vague rumors of the event two days after its occurrence (reported as an earthquake accompanied by a fall of stones), and although four hundred miles distant from the place, he immediately visited New Concord and obtained all possible data respecting the phenomenon. He is convinced from a thorough examination of the facts that no fall of meteoric stones before recorded possesses so many points of interest as the one in question, surpassing even the far famed fall at *L'Aigle*. He reserves many details of the event with his chemical examination for a paper in the next number of this Journal. The analyses, so far as they are complete, show the constitution of the New Concord stones to be identical with those that fell about the same time last year, March 28, 1859, in Indiana (see this Jour., xxviii, 409).

Louisville, Ky., June 6, 1860.

Thus far about thirty stones have been recovered from this fall, and one witness estimates the entire weight of the fragments discovered at about seven hundred pounds.

The fine specimen (figure 1) secured by Prof. Andrews for Marietta College, is, we believe, the largest meteoric fragment hitherto recorded as existing in one piece. Profs. Silliman and Kingsley estimated the weight of a fragment of the Weston meteorite (1807), which was dashed in many pieces by falling on a rock, as about 200 pounds.—Mem. Conn. Acad, i, 149.

ART. XIV.—*Review of Dr. Antisell's Work on Photogenic Oils, &c.*

[The following Review of Dr. Antisell's book on Photogenic Oils has been for some months in type waiting an opportunity when our other engagements would permit its publication. It will amply repay the careful perusal of all who are interested in this important practical subject.—EDS.]

REVIEW.

1. *The Manufacture of Photogenic or Hydro-Carbon Oils from Coal and other Bituminous Substances capable of supplying Burning Fluids;* by THOMAS ANTISELL, M.D., Professor of Chemistry in the Medical Department of Georgetown College, D. C., etc. etc. New York and London: D. Appleton & Co. 1859. pp. 144.—In entering an earnest protest against the work before us, we would not have our motives misunderstood. We are not of those who would condemn a book solely on the ground that it is "not so good as it should be," and will not therefore urge this objection against the effort of our Author, although it would be hard to find a case to which the charge would more forcibly apply. But we do condemn most heartily the presumption of the man who in these days attempts to write a handbook upon any scientific or technological subject with which he is not somewhat familiar. We believe, moreover, that errors, either of omission or of commission—accidental or intentional—in scientific writings, which exceed the well-understood conventional limits of *tolerance*, should not be allowed quietly to pass without correction.

Dr. ANTISELL, from his position of chemical examiner in the Patent Office at Washington, has naturally had a rare opportunity of familiarising himself with the recent improvements which have been made—or claimed—in the manufacture of coal oils. In the work in question, he has published an index of these, which cannot but be acceptable to all who are interested either in the practical or scientific consideration of the subject. Had this list been published by itself, or had it been incorporated with a portion of the materials which Dr. A. has now exhibited, in an article, or a short series of articles, in some one of our scientific or technological magazines, it would have been most gratefully received, and, we doubt not, widely copied. Diluted and scattered as this information has been, however, that it might fill a volume, its value has been lessened in no slight degree.

We have endeavored, in vain, to make out the point of view from which the Author regarded his subject. Claiming the attention of all persons engaged in the manufacture of liquid products from the distillation of mineral combustibles, his work is nevertheless not a didactic one. In it scarcely any attempt is made to instruct the manufacturer, either by a clear enunciation of general principles to be followed, or of special details to be observed in given cases;* while a most lamentable lack of familiarity with the chemistry of the subject is continually exhibited throughout the work. Indeed the book is simply a jumble of badly selected extracts, huddled together in a manner which must be anything but edifying to the student. As a compilation, it has the merit of directing attention to a number of sources from which valuable information may be derived; while it has the great fault of omitting to mention numerous other sources of knowledge of equal or of greater value.

In several instances, moreover, erroneous assertions are made, or wrongful conclusions drawn. One or two of these we propose to discuss and correct

* In this respect our author has fallen far below the level attained by previous writers upon the subject. Compare for example: UHLENHUTH, *Handbuch der Photogen-und Paraffin-Fabrikation.* Quedlinburg Basse, 1858.

in this article. Our attention will be especially directed to the first chapter of Dr. Antisell's book—"History of the Art"—for in it are errors which have too long been current in the annals of chemical science—errors, the repetition of which by our author is the more unpardonable, since, from his very position, he should have known them to be such. Indeed, from statements to be found in various parts of his work, it would appear that he must have known of these errors—that he must have been in possession of most of the facts which will here be brought forward.

That we may form a correct notion of the subject under discussion, let us here digress for a moment.

As a general rule, when any bituminous substance is subjected to distillation—in the ordinary acceptation of the term, *i. e.*, when it is gradually heated in any appropriate apparatus, a quantity of an oily fluid is produced, which may be collected in receivers; small quantities of gas, water, and other incidental products being at the same time obtained.

The oily liquid, which alone interests us here, known in this country as *crude coal oil*, is a mixture of various hydrocarbons, among which the wax-like substance Paraffine is an almost never-failing constituent. Crude oil, though of course varying greatly, according to the sources from which it is derived, like the various marketable "coal oils" obtained from it by purification, is specially characterized by its low specific gravity, being capable of floating upon water.

When, on the other hand, a bituminous substance, instead of being gently and gradually heated, is suddenly exposed to the action of an intense heat—when, as in the ordinary process of gas-making, it is thrown into vessels of iron or clay, which have *previously* been brought to a bright red heat, a different set of products is obtained. A large quantity of permanent gas is produced, while the liquids formed are no longer the light oily compounds just spoken of, but are composed of another set of hydrocarbons which taken collectively, are *heavier than water*. These constitute coal-tar. Among them paraffine is no longer found, excepting in comparatively rare instances, another solid substance, Naphthaline, being a characteristic component of the mixture. When the process to which the bituminous matter is subjected is a mixed one, *i. e.*, when a portion of the substance comes in contact with strongly heated surfaces, while other portions receive only an amount of heat sufficient to distill off oils of the kind first described, a mixed product, containing both coal-oil and coal-tar, is naturally obtained. As an instance of such mixed product may be mentioned the tar obtained in the preparation of gas from Boghead coal,* it being almost impossible, in this case, to maintain the retorts at the temperature best suited for gas-making, on account of the great amount of heat which is rendered latent by the enormous volume of gas generated by this highly bituminous substance.

It should be mentioned, that both crude coal-oil and coal-tar contain a quantity of "light stuff," composed of several exceedingly volatile and inflammable liquids. Some of these naphtha-like fluids, for example benzol—the benzine of the French—(known as *benzule* in the private vocabulary of Dr. Antisell, or that of his proof-reader)—may occur both in crude-oil and in tar; others do not. We refer to these "light-stuffs" here merely for the purpose of explaining that they have been at times spoken of as "volatile oils," from the resemblance which they bear to spirits of turpentine and other essential oils, and to eliminate them from the discussion. They are of but minor interest at the present moment, when compared with the true "coal-oil" now so largely employed in this country. We may mention, in passing, that Dr. Antisell has very inconsiderately obscured his historical sketch of the progress of the art of distilling coal-oil

* In the same class are several Scotch cannels, our own Breckenridge and allied coals, also the Albert coal of New Brunswick and the like.

by blending with it the question of coal-tar naphthas. He has, for that matter, been unfortunate throughout in the presentation of this part of his subject; all the crude liquid products of distillation, at whatever temperature the process has been conducted, being indiscriminately classed by him as *tar*. Now, it is well known to practical men, as has already been described, that the products obtained from bituminous matters by slowly distilling them, is as different from coal-tar as ether is from alcohol. The term crude-oil, by which the first-named liquid is known to manufacturers in this country, characterizes it perfectly; so does the term *huile de schiste* (written at times simply "*schiste*") of the French.*

It is surprising that Dr. Antisell should have followed the example of several German authors—without their excuse—in thus perplexing his readers.

In returning from this digression, we would expressly declare our disbelief in the adage which allows for the existence of no novelty. Still we do believe that very few of the arts have sprung into existence in a day, their perfection, and especially their development, having almost always resulted from the successive labors of numerous individuals; and we do believe that the inventor, who first practically "applies" any abstract knowledge, and thus creates a new art or branch of industry, is entitled to credit therefore—and to far more credit, and that of a different order, than the man who subsequently introduces this art into a foreign country. We would not detract from the efforts of the latter; on the contrary, would accord them high praise; but we desire, first of all, to see justice meted out to him who created the art—to those who increase human knowledge, sooner than to its mere diffusers.

We would therefore join issue with Dr. Antisell when, in his preface, he tells us that his book is a "record of the origin and condition of an infant art," and again mentions "this new branch of industry." So, also, in the first lines of his Historical Introduction, where he speaks of "the new and extensive manufacture of oils from coal and other bituminous substances." For these statements are not only erroneous in themselves, but they—no less than the unfair allusions which appear on subsequent pages—tend to do great injustice to earlier inventors, and especially to the memory of a man whose name must ever remain inseparably connected with the history of the *art* of manufacturing the fluid now known as coal or paraffine-oil. We refer to SELLIGUE. More than twenty-five years ago, this inventor's method of obtaining such oil was described in the *Journal des Connaissances Usuelles*, for Dec., 1834, p. 285. (See also *Dingler's Polytechnisches Journal*, 1835, lvi, 40.) This article was subsequently followed by numerous others, until in Selligue's patent of March 19, 1845, we find the whole subject treated of most fully and clearly. As a lucid and truthful description of his processes and of the products obtained, this specification is most praiseworthy. Few subsequent writers upon the subject have been able to add anything to the stock of knowledge which it imparts. Taken for all in all, it is doubtless the most meritorious essay which has ever been published upon the art of manufacturing coal-oil. We can but reiterate our statement, that the brief, inaccurate, and exceedingly superficial comments which have been bestowed by Dr. A. (pp. 9, 80, etc.) upon the information which Selligue has imparted in his admirable series of essays, does great injustice to the subject as well as to this author.

Leaving for a moment the minute consideration of Selligue's improvements, let us first glance at the labors of some of his predecessors.

As Dr. Antisell has truly said (p. 7), the discovery of the production of oil from coal appears to date as far back as the time of Boyle, (1728–1799), when the well known experiments of Dr. Clayton were made.†

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* We may here observe, that throughout this article we shall translate the French term *huile de schiste*, by its English equivalent, *coal-oil*.

† Philosophical Transactions, Jan. 1739, No. 452, p. 59; in Martyn's Abridgment, vol. ix. p. 395.

In distilling coal from a pit near Wigan in Lancashire, this observer obtained, first phlegm (water), then oil, and finally gas.

No doubt an earlier record of similar experiments might be found in the writings of the alchemists, who, as is well known, subjected almost every substance to processes of distillation.

During the last century attention was again several times called to the fact.*

It would seem, however, that nothing very definite was published before the year 1830. UNVERDORBEN† had, indeed, in the preceding year, called attention to oils distilled from petroleum, and even appears to have obtained paraffine—to which however he gave no name.‡ The attention of the scientific world was first really attracted to this substance by the memorable memoir of Reichenbach,§ who separated it, in the first instance, from wood tar, and described its properties at length. In the following year, Reichenbach‖ is at great pains to prove that the crude-oil, obtained by slowly distilling coal, contains no naphthaline,¶ that naphthaline is not a product of the slow distillation of coal, but is a result of the subsequent decomposition of such products by heat; and that the coal-tar of gas-works is not crude-oil, but an impure mixture of the products of distillation with those resulting from their decomposition.**

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* In addition to the authorities cited by Dr. A. (p. 8), we would mention the following from *An Experimental History of the Materia Medica, or of the Natural and Artificial Substances made use of in Medicine;* by WILLIAM LEWIS, M.B., F.R.S., 3rd Edit. 8vo, Dublin, MDCCLXIX, vol. ii, p. 143, Article *Petroleum;* also, (according to American Druggists' Circular, iv, 36,) in the London edition of Lewis. 4to, 1761, p. 436:

"Some mineral oils, procurable among ourselves, are used by the common people, and often with benefit. The empirical medicine, called British oil, is of the same nature with the petrolea; the genuine sort being extracted by distillation from a hard bitumen, or a kind of stone coal, found in Shropshire and other parts of England."

† *Berzelius's Jahresbericht,* x, 181, from *Kastner's Archiv,* xvi, 122; also in *Schweigger-Seidel's Journal für Chemie und Physik,* 1829, lviii, 243.

‡ For allusions to other earlier German researches bearing upon the subject, see Reichenbach's Memoirs, which will be cited directly. Compare also Gmelin's Handbook of Chemistry (Cavendish Soc. Edit.), xii, 439.

§ *Journal für Chemie und Physik,* (or *Jahrbuch der Chemie u. Physik, Band,* xxix) *von* Schweigger-Seidel. 1830, lix, 436.

‖ *Ibid,* (or *Neues Jahrbuch der Chemie u. Physik,* B. 1,) lxi, 175.

¶ Dr. Antisell dismisses this article (p. 11) with the statement that "in 1830-'31, Reichenbach discovered naphthalin." It may not be amiss to state that naphthaline was discovered at least ten years earlier, having been described by GARDEN in 1820 (Thomson's *Annals of Philosophy,* xv, 74), to whose labors as well as to those of CHAMBERLAIN, KIDD, and others, Reichenbach particularly refers in this very article. See also *loc. cit.* B. lxviii, [B. viii, of the "*Neues Jahrbuch,*"] S. 233.

** It must here be explained that Reichenbach has suffered great injustice at the hands of those who, in translating portions of his papers, have rendered his term "*Steinkohlentheer*" literally—coal-tar. Now the term coal-tar, in countries abounding in gas-works like England or the United States, means the tar of gas-works, and it means nothing else. Gas-works, it must be remembered, were, until quite recently, by no means so common in Germany, and were doubtless rare enough in 1830, consequently, it is not at all strange that the English idea of "coal-tar" should not have become current in that country. Reichenbach, for that matter, distinctly and repeatedly asserts, that his "*Steinkohlentheer*" is a very different substance from the tar of gas-works. In a word, it was *crude-oil.* If, perchance, there may be any person who would accuse us of mistranslating certain *words* used by Reichenbach, we would at once refer such an one to the orginal memoirs of this author. Submitting it to the judgment of any competent chemist, whether we have misinterpreted his lan-

These experiments were made upon a manufacturing scale, Reichenbach being, at this time, "chief of an extensive system of mines, iron furnaces, machine shops, chemical works, etc., most of them established by himself on the estate of Count Salm [Blankso, Moravia]. These works lie along a line some fifteen miles [5 *Stunden*] in length." (Schweigger Seidel).

In another article published later, in 1831,* he describes his method of obtaining paraffine from the distillation of flesh and of *coal* (portions of 75 lbs. weight having been operated upon). With regard to coal, he particularly urges the necessity of slow distillation, in order to prevent the decomposition of the first products and the consequent formation of naphthaline, as explained in his previous article, to which he refers. The paraffine was separated from the less volatile portions of the rectified oil by cooling—the description of which oil R. reserves for a separate article.† He also obtained paraffine from petroleum. Two more papers upon the subject were published by Reichenbach in this year,‡ only the first of which is of particular importance in this connection. It relates to *Eupion* (εὖ very, πίον fat). A term by which Reichenbach designates, in some instances, a portion, in others the whole of the somewhat difficultly volatile, fat-like oils, prepared by purifying the first product obtained by slowly distilling substances of animal or vegetable origin. This eupion was, in fact, a mixture of several hydrocarbons—the same which, in similar mixtures, are now collectively known in commerce as coal-oil; called paraffine oil by some, and designated in the retail trade by innumerable other names of only local significance.

Eupion was obtained by Reichenbach from the products of the slow distillation of animal and vegetable substances, as well as from coal, and was minutely described by him. We make but a single extract from this article, which occupies some thirty-two pages: "When any one shall succeed in separating eupion, at a sufficiently cheap rate, from the tars [crude-oils], it will very probably enter into the circle of substances useful in household economy. For, since it burns from a wick, brightly and clearly, and is free from smoke, it is in no wise inferior to the finest oil as an illuminating material. It does not grease nor crust the wick, nor stiffen when cold. If we consider, in addition to this, that for all purposes where cold can exert no influence, the paraffine need not be separated, but can be left dissolved in the eupion, and used in conjunction with it for lighting; we shall perceive that this is of some importance, since the two substances are thus mutually improved for technical purposes."

In 1832, Reichenbach§ again published a note upon eupion; and, in 1834, another long article,‖ in which he once more dwells upon its useful properties.

Reichenbach's contributions on the subject of the dry distillation of organic substances, are comprised in some twenty or more long articles, not counting

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guage, [compare, for example, *loc. cit.*, B. lxviii., [B. viii, of the *Neues-Jahrbuch.*,] S. 226].

It may be worth while also to call the attention of the reader to the fact that all of the substances discovered by Reichenbach in "tar" (as the text-books tell us) were in reality obtained from crude-oil. Knowing this, every one familiar with recent chemical literature, will perceive at once why so few of R.'s scientifiic results have been corroborated. For, until quite recently, the attention of chemists interested in such researches, has been almost completely occupied with the subject of coal-tar. Compare also Reichenbach's complaint against Dumas and Laurent, in Schweigger-Seidel's *Journal für Ch. u. Phys.*, 1838, lxviii, 223.

* *Loc. cit.*, lxi, [or B. 1, of the *Neues-Jahrbuch*], S. 273.

† *Vid. infra.*

‡ *Loc. cit.*, lxii, [or B. ii. of the *Neues-Jahrbuch*], S. 129, 273.

§ *Loc. cit.*, B. lxvi, [B. vi. of the *Neues-Jahrbuch*], S. 318.

‖ Erdmann's *Journal für praktische Chemie*, i, 377.

several smaller "notes." A tolerably complete list of which may be found in Erdmann's Journal für praktische Chemie, i, 1. It is very much to be regretted that these memoirs have never been collected and published as a separate volume. Even now, any chemist who could find time to collect these scattered articles and translate them into English or French, would unquestionably promote the cause of science by so doing.

Looking at the question for a moment, solely in its scientific bearings, we cannot refrain from an expression of astonishment, that the details of Reichenbach's researches are so little known to the generality of chemists;* while, on the other hand, we are forced to confess, that it is indeed rare that scientific researches, conducted by a chemist in his laboratory, have so fully described a future art—have so accurately pointed out the methods to be followed and precautions to be observed by the practical manufacturer. We must not omit to mention that, in 1831, Christison† of Edinburgh made known his discovery of paraffine in petroleum from Rangoon. Not knowing of Reichenbach's previous publication, Christison named it ***Petroline***, but subsequently admitted its identity with paraffine. In 1833, Bley‡ distilled oils from lignite.

A little later, in 1834, Gregory§ published an able article upon paraffine and eupion, and their occurrence in petroleum. Of this memoir we cite but two lines, (vid. Trans., p. 129, or Rep., p. 113). "It follows," says Gregory, "that there are some kinds of naphtha [petroleum] which contain paraffine and eupion, and are consequently the results of destructive distillation."

In the following year, v. Kobell‖ also noticed paraffine in petroleum.

For the labors of Hess in Russia, and of several other chemists in Germany, as well as for the interesting discussions which followed between these

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* This lack of information appears to depend upon the circumstance, that the writers of most recent chemical text-books seem to have derived their knowledge of the subject in question, from Gay Lussac's brief abstract of Reichenbach's earlier memoirs, which was published in 1832, in Poggendorff's *Annalen*, xxiv, 173; also in the *Annales de Chimie et de Physique*, [2], l, 69; and quite extensively copied by the journals of the day.

In this connection we would respectfully urge upon all those who have fallen into the common habit of regarding as somewhat apochryphal the numerous substances of greater or less scientific interest, which Reichenbach separated from the products of dry distillation, that before seeking to discredit—or allowing themselves to disbelieve—them, they should conduct experiments similar to his, *on a scale of equal magnitude*. Let us here also bear in mind the luminous conclusion of the late Dr. Hope of Edinburgh, who, as the story goes, (Vid. London Chemical News, i, 56), one day informed his class that Reichenbach had discovered in tar, "creosote, picamar, paraffine, cedriret, capnomor, and a host of other substances of no interest or importance whatever." Of these "unimportant" substances, two at least, eupion and paraffine, are to-day as well known, in the world, as bees-wax or spermaceti, although comparatively little—we had almost said nothing—has been added to the scientific knowledge of them, since the publication of Reichenbach's memoirs. If, perchance, any other of these well-nigh forgotten bodies should be found to possess any technical importance, we would quickly enough find some one claiming credit for its "discovery," and oppressing chemical nomenclature, by adding yet another name to the existing "host." Even now we await, with no little interest, the elucidation of the question—whether the new violet dye, prepared by oxydizing anilin, which is exciting so much interest, under the names anilein, Perkin's purple, *mauve*, etc., is not identical with, or a component of, the *pittical* of Reichenbach.

† Transactions of Royal Society of Edinburgh, xiii, 118; also in Repertory of Patent Inventions, 1835, [N. S.] vol. iii, p. 390.

‡ Schweigger-Seidel's *Journal für Chemie u. Physik*, B. lxix, [B. ix, of the *Neues-Jahrbuch*], S. 129.

§ *Transactions of Royal Society of Edinburgh*, xiii, 124; also in *Repertory of Patent Inventions* 1885, [N. S.] vol. iv, p. 109.

‖ J. pr. Chem. v. 213.

observers and Reichenbach, the reader may consult the general index [Namen-u. Sach-Register zu den Bänden i. bis lx, Leipzig, 1845] to Poggendorff's Annalen der Physik u. Chemie.

At the same time that these scientific researches were in progress in Germany and Scotland, or even earlier, numerous practical efforts to manufacture oils from bituminous substances were made in France.

Although the precise date at which these experiments were commenced is somewhat obscure, it will not be difficult to trace the history of the successful development of the industry to which they gave rise.

As stated by Dr. Antisell, the MM. Chervan* had a patent, dated in 1824, for distilling bituminous substances. Blum and Moneuse,† in 1832, claim only the application of coal-oil to purposes of lighting—not its manufacture, which they allude to as being well known.

Subsequently (7th October, 1833) Boscary‡ obtained a patent for extracting pyrogenous oil from different substances, asphaltums, etc., and especially from the shales which occur in the environs of Autun (*Saone* et *Loire*), and finally from all the bituminous matters in France. The oil, which is obtained by distilling the shale in metallic cylinders, may be used, according to Boscary, instead of fish-oil or resin, for gas-making—a much better gas than that prepared from coal being thus obtained.

In 1833, Laurent§ occupied himself with the investigation of various bituminous shales, both French and English, at the instance of the MM. Blum, whom he mentions as being occupied with the distillation of oil from the shales of the environs of Autun.‖ Laurent gives the details of the process employed by himself, telling us that the retort in which his shales were distilled attained a sombre red heat at the close of the operation; also of the percentage amounts of oil (20 p. c.), gas, coke and water obtained from the Autun shale; how the oil cannot be burned in ordinary lamps, on account of smoking, but affords a very luminous flame when consumed in lamps furnished with suitable chimneys. He shows moreover that the oil contains paraffine, and does not contain naphthaline.

Laurent subsequently published another paper¶ upon this oil, in which article he records his efforts to ascertain what definite chemical compounds are contained in the oil. One of the products obtained by fractional distillation, viz., an oil boiling at 167° to 170° (C.) = 333° to 338° F., he considers as identical with eupion.

In 1834, we find, for the first time, an article** describing the process of Selligue, although it would appear from the statements of this chemist and of others, that his attention had been directed to the subject of distilling bituminous shales several years earlier. The cited article relates how the shale is *slowly* distilled in iron cylinders, until no more oil comes over; how the oil obtained is characterized by containing neither oxygen nor naphthaline, but a solid substance differing from the latter, and resembling that called paranaphthaline†† by Laurent.

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* Brevets d'Invention xviii, 232. † *Ibid.* lxv. 250.

‡ *Ibid.* lxviii, 359.

§ *Annales de Chimie et de Physique*, liv, 392.

‖ According to Laurent, he had himself proposed to a company, in 1829, to work these shales, in order to extract the oil contained in them, and to employ it for lighting.

¶ *Comptes Rendus*, 1837, iv, 909; more fully in *Annales de Chimie et de Physique*, lxiv, 321.

** *Journal des Connaissances Usuelles*, Dec. 1834, p. 285; also in Dingler's *Polytechnisches Journal*, 1835, lvi, 40, from which our extract is taken.

†† The inadvertency of confounding this body with paraffine was subsequently corrected by Selligue.

In 1834, '35 and '36, Selligue* was principally occupied with his well-known process for making water-gas. In calling the attention of the French Academy† to this, he remarks that, in conjunction with David Blum, he holds a patent granted in 1832 for the application of oils obtained from shale to purposes of direct illumination, and that the working of the shale employed is in the hands of a company capable of developing the business to any extent which commerce or the arts may require.

In the same year Payen,‡ in reporting upon Selligue's water-gas, remarks upon the great importance of the new industry of distilling oil from shales which S. has introduced.

In the following year we again find Selligue before the Academy§ requesting that body to appoint a committee to examine the merits of his new system of gas-lighting; his process of distilling bituminous shales on the great scale by means of apparatus, each one of which furnishes from 1,000 to 1,400 pounds of crude oil per day—this being about 10 per cent of the weight of the shale employed, and being almost all which exists in the raw material; also of his process of separating various products from the crude oil, some of which are applicable to the production of gas, others to ordinary purposes of illumination, and others to different uses in the arts. This petition was referred to a committee of three—Thenard, D'Arcet and Dumas—who reported in 1840.‖ They mention the localities of Selligue's three establishments for obtaining oil from shales; the amounts of oil obtained from the different kinds of shale, &c.

In 1838 Selligue also obtained a new patent¶ "for the employment of mineral oils for lighting." In his specification he informs us that the principles upon which his processes for rendering the oil obtained from shales proper for the purposes of direct** illumination depend, are:

I. Removal of almost all odor. II. Removal of all tar. III. Removal of the most volatile portions of the oil, which are also the most inflammable and the most odorous, the presence of which would cause the oil to have too great fluidity for the capillarity of ordinary wicks. * * *

The operations, continues Selligue, consist in slowly distilling the bituminous shale, and collecting the liquid products in large receivers. These products are redistilled, and divided into fractions by refrigerating. They are treated with concentrated sulphuric acid for a longer or shorter time according to the nature of the shale employed. Twenty-four hours are ordinarily sufficient, the oil being agitated from time to time. The quantity of acid used varies from $\frac{1}{10}$ to $\frac{1}{20}$. After this the oil is to be carefully drawn off from the tar, and washed with water. Slaked lime is then added and a current of steam passed through the oil in order to carry off by distillation all the more volatile and odorous liquids. This last, says Selligue, is the most important part of my process, for if this very inflammable portion were allowed to remain in the oil, one could not use the latter in ordinary lamps *à courant d'air*. * * * This patent it should be observed claims only to be an improvement upon that of Blum and Moneuse (*vid. Supra*). Selligue asserts, however, that coal-oil had never before been prepared in such a manner that

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* See seven patents in *Brevets d'Invention*, lxx, 269. Of these patents two are dated 1834; two, 1835; and three, 1836. For a description of his process of gas-making see also *Bulletin de la Société d'Encouragement*, Oct. 1838, p. 396; or Dingler's *Polytechnisches Journal*, lxxi, 29.

† *Comptes Rendus*, 1837, iv, 969.

‡ Dingler's *Polytechnisches Journal*, lxviii, 201; from *Bulletin de la Société d'Encouragement*, Dec. 1837, p. 493.

§ *Comptes Rendus*, 1838, vii, 897.

‖ *Comptes Rendus*, x, 861; also in Dingler's *Polytechnisches Journal*, lxxvii, 137.

¶ *Brevets d'Invention*, lxviii, 395.

** The term "direct illumination" is constantly used by Selligue in contradistinction to the indirect use of the oil in his process of gas-making.

it was fit for use in common lamps. This has, indeed, he says, been the subject of many researches, but no one has hitherto succeeded in avoiding the empyreumatic odor, and the very inflammable products which caused the oil to rise too quickly to the summit of the wick. He goes on to define the difference between his purified oil and the crude oil obtained directly from shale. On the 27th of March, 1839, Selligue specifies certain additions and improvements to the preceding patent. I should add, he says, that I now divide the products of distillation into four distinct parts, which afford me every facility for employing these products in the arts and for domestic economy. In these divisions there are indeed some anomalies which arise from differences in the shales, &c. which I treat; but the following products are always obtained:

I. A light, volatile oil more or less odorous according to the source from which it is derived. * * * This can be used in painting, for dissolving resins, &c., for lighting by vaporising it (it being very volatile) or for the production of gas according to my system.

II. A fat oil only slightly volatile, and having but little odor; this can be used for domestic purposes in ordinary lamps with or without admixture of animal or vegetable oils.

III. A fatty substance almost odorless, possessing all the properties of the fats, and well adapted for use in the arts. It can also be used for lighting, either by mixing it with light oils or by decomposing it for the production of gas. It can moreover be used for soap* since it saponifies very well, and being without odor affords a very good soap; with ammonia the fat forms a sort of pomade.

IV. An odorless pitch of great purity and tenacity, suitable for preparing a black solid varnish for preserving wood, iron-work, &c. * * *

In 1839, Selligue† in alluding to the use of his oils in the treatment of cutaneous diseases speaks of the three large establishments for the distillation of bituminous shale which he has erected in the Department Saone et Loire, and mentions the fact that the oil (crude?) is furnished at the rate of about two cents [ten centimes] per pound.

The question of price is again discussed a few years later, when Selligue‡ says: it has been stated that crude shale oil costs only $1 50 per 100 pounds, and that it contains 60 per cent of a very light volatile ethereal oil well suited to afford light, as well as 40 per cent of a fat substance. Now since 1837, I have extracted more than 4,000,000 pounds of oil from bituminous shale, but the oil (crude?) costs 20 cents a gallon (22 frs. the hectolitre) or even 27 cents when delivered in Paris. From every hundred measures of the crude oil are obtained (by distillation) 20 measures of volatile oil boiling at 100° C.=212° F.; 30 measures of less volatile oil boiling at 150° to 260° C.=302° to 500° F.; 14 measures of an oil containing paraffine, and 28 measures of fat—five measures being lost. In purifying these products a further portion is lost.

The clearest of all Selligue's specifications, however, is that of the patent granted to him March 19, 1845§ for the distillation of bituminous shales and sandstones.

After describing the various forms of apparatus used in distilling, into one of which superheated steam was introduced, he enumerates the products of distillation as follows: I. A white, almost odorless, very limpid mineral oil—somewhat soluble in alcohol—which may be used as a solvent, or for purposes of illumination in suitable lamps, &c.

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* This "soap," (emulsion) is described more fully in the sequel.

† *Comptes Rendus*, ix, 140; also *Annalen der Pharmacie*, von Wœhler u. Liebig, xxxii, 123.

‡ Dingler's *Polytechnisches Journal*, xci, 193; from the *Moniteur Industriel*, 1843, No. 770.

§ *Brevets d'Invention*, [new series, (loi, du 5 Juillet, 1844,)] iv, 30.

II. A sparingly volatile mineral oil of sp. gr. 0·84 to 0·87, of a light lemon color, perfectly limpid, almost odorless, never becoming rancid, and susceptible of being burned in ordinary oil lamps, of constant level *à reservoir superieur*, with double current of air—a slight modification of the form of the chimney and burner being alone necessary. This oil can also be mixed with the animal or vegetable oils. Oils thus prepared do not readily become rancid, nor do they congeal easily when subjected to cold.

III. A fat mineral oil, liquid at the same temperature as olive oil. This oil contains a little paraffine; it is peculiarly well adapted for lubricating machinery, and has an advantage over olive and other vegetable oils, or neats-foot oil in that it preserves its unctuosity when in contact with metals and does not dry up. It saponifies easily, and forms several compounds with ammonia.

IV. From the oils Nos, I, II, and III, I extract a red coloring matter which can be used in various arts.

V. White crystalline paraffine which needs but little treatment in order to be fit for making candles; this substance does not occur in very large proportion in the crude oil, and the proportion varies according to the different mineral substances upon which I operate. There is but little of it in petroleum, and in the oil obtained from bituminous limestone. I often leave a great part of the paraffine in the fat oil and in the grease in order that these may be of superior quality.

VI. Grease. This grease is superior to that of animals for lubricating machinery, and for many other purposes, since it does not become rancid, and remains unctuous when in contact with metals.

VII. Perfectly black pitch—very "drying"—suitable for preserving wood, metals, &c.

VIII. An alkaline soap obtained by treating the oils with alkalies.

IX. Sulphate of ammonia. X. Manure prepared by mixing the ammoniacal liquor, or the blood of animals, with the crushed fixed residue (coke) of the shale. XI. Sulphate of alumina from the residue of the shale. F. H. S.

[*To be concluded.*]

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On Chlorophyll.*—Frémy has communicated the results of an interesting investigation of the green coloring matter of plants. His conclusions are as follows:

1. Chlorophyll may be decomposed into a blue and into a yellow substance.

2. These substances give compounds with alumina which are insoluble in water. When hydrate of alumina is added to an alcoholic solution of chlorophyll, neither of the constituents of the latter unites with the earth. When, however, water is added gradually, the blue substance and a little of the yellow first unite with the alumina to form a dark green precipitate, while the alcoholic solution remains yellow. Both bodies are precipitated together from a very dilute aqueous solution.

To separate the blue and yellow substances completely, the author introduces a mixture of two parts of ether, and one part of muriatic acid, diluted with a little water into a bottle, and shakes the whole till the mu-

riatic acid is saturated with ether. The natural chlorophyll which may be extracted from leaves by means of alcohol, may be easily decomposed into its constituents by shaking it with this mixture of muriatic acid and ether. The liquid first takes a brown color, after which the aqueous muriatic acid takes a beautiful blue, while the ether separates, holding the yellow coloring matter in solution. Frémy calls the blue substance phyllocyanin, and the yellow phylloxanthin. When alcohol is added, so that the two liquids mix, a green is again produced, of the color of the natural chlorophyll.

Young leaves exposed to the vapors of muriatic acid, quickly assume a beautiful green color. The yellow autumn leaves, on the other hand, contain only phylloxanthin. Under certain circumstances, phyllocyanin is capable of yielding another yellow substance, to which the author gives the name of phylloxanthein; from this substance phyllocyanin may be again obtained. The blue coloring matter of chlorophyll is more easily altered than the other. The author promises a further investigation of this very interesting subject; his results are of equal interest to naturalists and chemists.—*Compt. Rendus*, l, p. 405.

2. *On the Separation and Estimation of Phosphoric Acid.*—Chancel has given a method of estimating phosphoric acid, which depends upon the entire insolubility of phosphate of bismuth in liquids containing free nitric acid. When a solution of acid nitrate of bismuth, so dilute as not to be rendered turbid by water, is added to a liquid containing a phosphate dissolved in nitric acid, a very dense, white precipitate is immediately formed, which has the formula BiO_3, PO_5, and the constitution of which, according to Chancel, is perfectly constant.

The neutral phosphate of bismuth is perfectly insoluble in water, and dilute nitric acid, whether cold or hot; it dissolves sensibly in solutions containing a large proportion of salts of ammonia. The filtration and washing is very easy and rapid; the dried salt may be ignited in a platinum crucible, and does not fuse at a red heat.

Pyrophosphoric acid is also precipitated completely by the acid nitrate of bismuth; a white precipitate is formed which is much more voluminous than the tribasic phosphate. This precipitate has the formula $2BiO_3.3pPO_5$: it is easily and completely transformed into the tribasic phosphate by simply boiling it in the presence of an excess of the acid nitrate of bismuth.

The metaphosphates behave in the same manner, but the precipitate requires a longer boiling to be completely transformed into the ordinary phosphate.

Chancel asserts that he has been able to detect the presence of one milligram of phosphoric acid when mixed with 120mm. of alumina in a dilute solution containing more than a gramme of free nitric acid. As the precipitation is very rapid in a hot solution, and as the liquid becomes almost instantly clear, it would be easy to estimate phosphoric acid by a titrated solution of acid nitrate of bismuth.

Chancel prepares the acid nitrate by dissolving, with the aid of heat, one part of pure crystalline subnitrate of bismuth in four parts of nitric acid of specific gravity 1·36, adding to the solution 30 parts of distilled water, and filtering if necessary. In using this solution, the phosphate,

if not soluble in water, is to be dissolved in nitric acid, avoiding a large excess. The solution is to be diluted with water, the nitrate of bismuth added as long as a precipitate is produced, and the whole brought to boil, filtered and washed with boiling water. After drying, the filter must be separated as completely as possible from the precipitate, burned by itself, and the ashes added to the precipitate, which may be heated to redness in a platinum crucible. The bases are easily estimated in the filtrate, after removing the excess of bismuth by sulphuretted hydrogen. This process requires that the liquid should be free from chlorids and sulphates, which, when present, are easily removed by nitrate of silver and chlorid of barium.—*Comptes Rendus*, l, p. 416.

3. *On a New Mode of Preparing Calcium.*—Caron has succeeded in preparing large quantities of calcium by the following process: A mixture of 300 parts of fused and pulverized chlorid of calcium with 400 parts of granulated distilled zinc and 100 parts of sodium in pieces is to be heated to redness in a crucible. The reaction is feeble, and after some time flames of zinc appear. The heat is then to be moderated, the temperature remaining as high as possible without volatilizing the zinc; after a quarter of an hour the crucible may be withdrawn from the fire. It contains a well fused metallic mass, which is highly crystalline, and which contains from 10 to 15 per cent. of calcium. The alloy is then to be placed in a crucible of gas-retort carbon and the zinc expelled by heat: in this manner Caron obtained masses of 40 grammes at a single operation, and containing only the impurities of the zinc employed. As thus obtained calcium has a brass-yellow color and a density of from 1·6 to 1·8, it is not sensibly volatile, but filings of the metal burn with red sparks of remarkable beauty without formation of vapor, which seems to show that the metal is not volatile at the temperature of its combustion. The author promises to communicate the results of similar experiments in the preparation of barium, strontium, &c.—*Compt. Rendus*, l, p. 547.

4. *On a New Metallic Element.*—Von Kobell has discovered in euxenite, æschynite, and samarskite, and a tantalite from Tammela, a new metallic acid belonging to the same group with tantalic and niobic acids. To the new metal contained in this acid, the author has given the (not very well selected) name of *Dianium*. When dianic acid, as precipitated by ammonia from its solution in chlorhydric acid, is boiled with chlorhydric acid and metallic tin, a beautiful deep sapphire blue solution is produced, which remains blue after filtration. When tantalic acid, from the tantalite of Kimeto, or niobic acid from Bodenmais, are treated in the same way, the solution becomes bluish; and on adding water, the color quickly vanishes, and the solution, on filtering, passes through colorless.

When dianic acid is boiled with chlorhydric acid and zinc, instead of with tin, the blue solution does not appear, the precipitated acid becomes blue, but filters colorless, and is decolorized by water without being sensibly dissolved. When equal quantities of dianic, tantalic, and hyponiobic acid are boiled with concentrated chlorhydric acid, upon a funnel of platinum-foil for three minutes, all three give yellowish milky liquids; if water be then added, the solution of dianic acid becomes perfectly clear, while the tantalic and hyponiobic acids remain undissolved.

When freshly precipitated dianic acid is heated to boiling with dilute sulphuric acid, the milky liquid poured into a glass, and grains of distilled zinc thrown in, the dianic acid in a few moments becomes smalt blue, even dark blue, and retains this color for some time on addition of water; but the liquid passes through the filter colorless. In this respect, dianic acid resembles hyponiobic acid, while tantalic acid, under the same circumstances, becomes pale blue, and immediately loses this color on addition of water. In this manner, tantalic may be distinguished from dianic and hyponiobic acids. The relations of the three acids to chlorhydric acid and tin, and to sulphuric acid and zinc, are thus sufficient to distinguish them from each other.

Dianic acid appears to exist, though in a less pure state, in the tantalite from Greenland, in pyrochlore from the Ilmengebirg, and in the brown Wöhlerite—though the author had but small quantities of these minerals at his disposal. A small piece of black yttrotantalite, believed to be from Ytterby, gave the reaction of dianic acid. A second specimen, however, the specific gravity of which was found to be 5·55, contained tantalic acid.

Titanic acid is easily distinguished from the other acids of the same group, by boiling it with muriatic acid and tin, and diluting the solution with water. The blue color then passes to rose red, and the solution retains this color several days. When dianic acid is present, the blue color predominates, but after standing some hours the rose color of titanic acid appears.

The tantalite from Tammela, which Von Kobell terms dianite, has a specific gravity of 5·5—while the other tantalites vary in density from 7·06 to 7·5. The streak of dianite is dark grey, while that of the tantalites from Tammela is dark brown red. Before the blowpipe, dianite exhibits no sensible difference from the tantalite of Kimeto.

H. Rose, to whom Von Kobell sent a specimen of dianic acid for examination, considered it probable that the peculiar reactions of this substance might be due to the presence of tungstic acid. Von Kobell has, however, shown by special experiments, that this is not the case. In conclusion, the author recommends those who desire to repeat his experiments, to employ the same proportions of water, acid, etc., of which he himself made use, and for the details of which we must refer to the original paper.—*Bull. der Acad. der Wissenschaften*, March 10th, 1860, (Munich). W. G.

5. *Note on the extent to which Mercury volatilizes along with the vapor of water at* 100° C.; by J. W. Mallet.*—In Berzelius' Traité de Chimie it is stated that Stromeyer drew attention to the fact of the evaporation of mercury in considerable quantity at 60° to 80° C. with the vapor of water, the more volatile substance carrying with it the less volatile, as in the case of a solution of boracic acid when heated.

This fact does not seem to have been very generally noticed by the compilers of chemical text-books in treating of the history of mercury, though it is always stated that the metal is capable of volatilizing *to a very slight extent*, even when alone, at the common temperature of the atmosphere. Some doubt too would seem to have been thrown upon Stromeyer's observation by the experiments made, under peculiar condi-

* Communicated by the author.

tions, by Fresenius, and reported by him in the appendix to his treatise on quantitative analysis. It was found that 6·4402 grm. of mercury, *covered* with a considerable quantity of water, and heated to the boiling point of the latter for a quarter of an hour, lost but ·0004 grm., while exposure to the air at summer heat for six days produced a further loss of ·0005 grm.

I have lately observed the tolerably rapid evaporation of the metal with steam, produced not from a mass of water covering the quicksilver, but from a porous and highly absorptive clay which was dry to the touch, but gave off some eight or ten per cent of water when heated to 100° C. A specimen of this soil had been in contact with mercury, and several grams of the metal in small globules had become mixed up with the mass. It was placed in the common copper box with double sides which serves as a steam-bath, and exposed to the temperature of boiling water. In half an hour I was surprized to see that the little piece of glass tube through which the heated air and vapor from the inside of the steam-bath escapes was coated with a bright specular deposit of metallic mercury. This was brushed off, and the tube replaced, when again it became in a short time similarly coated. The steam-bath was kept in constant use in drying this and other specimens of soil for twenty or thirty hours, and in this time between four and five grains of mercury was collected from the glass tube. Doubtless the condensation of the mercury vapor on the latter must have been far from complete—a large proportion of mercury probably escaped into the atmosphere. At any rate it seems clear that the metal can volatilize in very considerable amount when surrounded by vapor of water at 100° C., and not at the same time pressed upon or affected by the cohesion of a mass of liquid water. Hence an obvious necessity for thoroughly effective condensation when mercury is to be determined in its compounds by ignition with soda-lime.

TECHNICAL CHEMISTRY.

6. *Disinfectants.*—The use of a mixture of coal-tar and plaster-of-Paris for purposes of disinfection and for dressing wounds, as proposed by CORNE and DEMEAUX (*Comptes Rendus*, xlix, 127; see this Journal, xxviii, 425), has been recently reported upon in the French Academy by a committee—Chevreul, J. Cloquet, and Velpeau (*rapporteur*)—to which the subject was referred in July, 1859.

The great interest, which this method,—so favorably commented upon by the distinguished surgeon Velpeau soon after its publication,—excited among the medical men of France gave rise to the publication of numerous other systems of disinfection, which being submitted to the Academy for its approval were also referred to the committee in question. The labors of its members have thus been materially increased, and their report swelled to the dimensions of a general treatise upon disinfectants—especially those applicable to wounds.

In numerous experiments made at the Hospital *de la Charité* the mixed coal-tar and plaster of Corne was exployed, both in the state of powder and as a poultice made by mixing it with oil. When applied as a thick layer, three or four times a day, upon putrid, gangrenous and sanious wounds, the powder destroyed their odor without giving rise to any special pain. Upon indolent sores, however, or upon recent burns,

the contact of the powder produced considerable smarting upon some patients, though well borne by others. Wounds of the first class were often found to be cleaned as well as disinfected; while those of the second class generally acquired a dirty, pale, gray tint, their circatrization being hindered.

The poultices were found to be more advantageous than the powder in the treatment of cavernous wounds, purulent or fetid, and sinuous foci, open suppurating abscesses, anthracoidal suppurations, etc.

Applied directly to the sore, the poultices destroyed the putrid odors, allayed the inflammation without augmenting the pain, leaving beneath them a healthier pus, and the surfaces in better condition. In a word, the mixed coal-tar and plaster, when properly applied, disinfects wounds and putrid suppurations. As for the absorbent and detergent qualities which its inventors also claim for it, these are less clearly evident.

The powder absorbs better than the poultices,—the latter, it is true, take up a portion of the morbid exudations, but unless the dressing is carefully renewed, five or six times a day, pus will nevertheless collect beneath it. From this it follows that after having been somewhat cleansed the wound ceases, at the end of a few days, to clean itself, or to heal more rapidly than it would with the usual topical applications.

Upon ulcerated cancers, the mixture, either as powder or poultice, disinfects them partially, but neither dries up the suppuration nor alleviates the pain.

It is in the dissecting-room, upon organic matter in a state of putrefaction, that the mixed coal-tar and plaster is all powerful. The most infectious masses, when imbued with the powder, or simply rolled about in it, lose at once their disagreeable odor. According to Velpeau, his autopsy room was as approachable towards the close of last summer as it had formally been repulsive. It was freed from flies and other insects, as well as from putrid odors.

Although it would have been out of the province of the committee to experiment upon the application of this mixture in disinfecting filth upon the great scale, they have nevertheless proved that it can be advantageously used in hospitals for deodorizing urine or fecal matters.

The following inconveniences, to which the use of the mixture in surgery would give rise, are enumerated:

It not only soils the clothes of the patient, but hardens them and causes them to weigh more heavily upon or about the wound; it imparts to the bandages, with which the poultices are covered, a very tenacious rusty or yellow color; it must be frequently renewed, and although it destroys putrid smells, it retains a bituminous odor by no means agreeable to many persons.

These inconveniences are of comparatively slight importance, it is true, and may possibly admit of being remedied.

Of the other disinfectants submitted to the committee, several were only modifications of that of Corne and Demeaux:—Vegetable tar, as shown by Renault, may be substituted for coal-tar.—A mixture composed of hydraulic lime and tar did not disinfect wounds to which it was applied, nor could it be supported by the patients. With regard to the assertions of some practitioners, that common earth, talc, flour, or other

vegetable and mineral powders—even poudrette—when mixed with coal-tar furnish a more convenient and less costly disinfectant than that prepared with plaster, the experiments of the committee have proved that while coal-tar, mixed with common earth, well dried, or with sand, is equally, or perhaps much more, efficacious for disinfecting fecal matter as when mixed with plaster; that while comparative experiments made from this point of view upon sulphate of lime, clay, charcoal, linseed meal, and earth have resulted in favor of the latter, the same is by no means true in surgery. When applied to wounds or infectious suppurations these different mixtures were only partially successful, having proved to be less efficacious than the mixed plaster and coal-tar. In like manner the proposal to use an emulsion of coal-tar and tincture of saponine has not been found advantageous in practice: most patients complained of it, their wounds exhibited scarcely anything satisfactory, while the disinfection was very imperfect. The mixture of plaster and coal-tar was substituted for it, upon the same wounds, with decided advantage.

Although the modifications of Corne and Demeaux' process have not been particularly felicitous thus far, they have nevertheless served to confirm the fact that in reality it is the coal-tar which acts the principal part as disinfectant in these various mixtures.*

Among the numerous other substances proposed as disinfectants, or for dressing wounds, the following have not afforded satisfactory results:

Chlorate of potash,—mixed with clay or kaolin (for example, 10 parts of chlorate to 90 parts of white clay or fine sand) which was proposed as an absolute disinfectant, neither disinfected nor absorbed the pus of fetid wounds. The mixture would be in any case much more costly than coal-tar and plaster and certainly less efficacious.

Whites of eggs,—mixed with chalk and applied to wounds, previously oiled, succeeded no better than simple-cerate.

* The inefficiency of sulphate of lime as a general disinfecting agent when used by itself may be readily demonstrated by the following experiment which is of interest in view of the fact that a belief in the utility of gypsum as a deodorizer appears to be widely spread among recent writers. For that matter we are told by Paulet (*Comptes Rendus*, xlix, 199) that during the last 25 years more than fifty authors of processes of disinfection have announced, each as he believed for the first time, the use of plaster as a means of disinfection.

If a mixture of about equal volumes of powdered gypsum and fresh urine be introduced into a small phial, the mixture placed in a warm room and thoroughly shaken several times a day until the urine has become putrid, it will be observed that an exceedingly disagreeable odor will be developed, differing from that of ordinary stale urine inasmuch as it is unalloyed with the odor of ammonia. For the complete success of this experiment it is important that a large excess of sulphate of lime should be present and that the mixture should be frequently agitated, else the whole of the carbonate of ammonia will not be decomposed and will tend to mitigate the fetor of the special odor of the putrid urine. So far from disinfecting in this case the sulphate of lime really destroys a deodorizing, or at least a masking agent, ammonia; leaving free,—purified as it were, and unadulterated, an odor, the peculiar offensiveness of which is remarkable. Sulphate of iron being substituted for gypsum in this experiment afforded a somewhat similar result, although the odor obtained was a trifle less insufferable than that of the experiments with sulphate of lime. It should be here mentioned that the odors in question were in no instance contaminated with sulphuretted hydrogen,—as was ascertained by careful trials. F. H. Storer.

Powdered sugar.—When employed in layers upon ulcers forms crusts, beneath which the suppurations accumulate and hinder the process of healing.

Cherry-laurel water, glycerine and cellulose.—According to Antier, glycerine mixed with equal parts of cherry-laurel water forms a valuable absorbent or disinfectant to be applied as a lotion or injection. This mixture converted into pomade by mixing it with powdered almonds was also proposed as a topical application for all kinds of wounds. But in the hands of the committee neither the liquor nor the pomade by themselves or mixed with kaolin produced any effect more marked than that of lead-cerate and other anti-putrid or detergent solutions already in use.

The members of another group of disinfectants are worthy, in various degrees of consideration.

Among these charcoal appears in the front rank.—Surgeons have long regarded it as one of the best antiseptics known. Confined between pieces of linen according to the process of Malapert and Pichot it is more readily applied than when used as powder directly upon wounds; but the mixture of coal-tar and plaster, which disinfects still better and is more cleanly, is susceptible of a simpler and a more general application.

Coke of Boghead coal,—in powder as proposed by Moride* like carbon

* In view of the claim of Moride (*Comptes Rendus*, xlix, 242) as well as from its general interest the following extract from a report made to the British Secretary of War by Lewis Thompson (London Journal of Gas Lighting, Water Supply and Sanitary Improvement, 1856, v, 11) may here be cited.

Mr. Thompson states that he has instituted a set of experiments having a purely money basis as their exponent.—The articles enumerated were each employed until they practically deodorized one uniform quantity of the same mass of putrid sewage and the money value of the proportions thus used was deduced either from a broker's price-list or, where this failed to give the requisite information, by special inquiry from a wholesale dealer. The amount of sewage operated upon in each experiment was half a gallon taken from a single tank which had been recently filled out of a large and very offensive ditch or open sewer. Two indications of the progress of the disinfection were had recourse to in these experiments; one with paper dipped in sugar of lead which gradually ceased to become brown as the deodorizing agent was added in successive portions; the other had reference to the discontinuance of any offensive smell; and the attainment of this last condition was regarded as the termination of each experiment.

By this means he was enabled to draw up the subjoined table which shows at a glance the comparative cost of executing the same amount of deodorizing work with each agent on the supposition that Boghead charcoal can be had at the rate of $3·00 [=12s.] per ton.

Table showing the cost of Purifying one uniform Quality of Feculent Sewage by the several Articles mentioned.

Boghead charcoal (coke),	$3·00
Nitric acid,	8·50
Black oxyd of manganese,	9·25
Chlorid of lime,	10·75
Peat charcoal,	11·00
Subchlorid of iron (imperfect),	11·25
Animal charcoal,	16·75
Chlorid of manganese (imperfect),	17·50
Bichlorid of mercury,	18·00
Impure chlorid of zinc in damp powder,	26·00
Chlorid of zinc in solution as patented by Sir Wm. Burnett,	37·00
Sulphate of copper,	39·00

The sulphates of zinc, iron, and alumina; common gypsum; sulphuric, sulphur-

when employed comparatively with coal-tar and plaster, alternately upon the same patients, proved to be less efficacious, less convenient and more disagreeable than the latter.

Mixed plaster and charcoal,—proposed by Herpin of Metz, irritates the wounds, disinfects badly, and soils everything it touches.

Carbonic acid,—proposed by the same author, appears to the committee to be too difficult of application in practice, though theoretically founded upon important analogies.

Bituminous Water of Visos—proposed by Manne, and the mud of rivers used as a poultice by Desmartis, do not appear to be susceptible of being substituted for the mixture of Corne and Demeaux.

The following substances have long ago acquired a place, each in its own way, in the class of disinfectants.

Tincture of iodine has been employed as an antiseptic by hospital surgeons since 1823. By modifying the surfaces to which it is applied, it usually improves the appearance of the pus, lessens its acridity, and is, to a certain extent, antagonistic to putrid infections. It disinfects, however, only incompletely, causes severe pain when applied to open wounds, and would be expensive if used on a large scale; finally, the odor of iodine is neither agreeable nor unattended by inconveniences.

Perchloride of iron has been used for some twelve years in hospitals as an antiseptic and as a means of modifying certain wounds, and putrid or sanguineous foci.—Without diffusing the disagreeable odor of tincture of iodine, it has, like the latter, the fault of disinfecting badly, of causing much pain, and of acting violently upon the diseased tissues, besides injuring the cloths which are soaked in it even more than is the case with the coal-tar and plaster.

Both iodine and the salt of iron just mentioned, are in fact agents of another order; they have rendered, and do still render important services. They are certainly well worth preserving, but should not be compared with the mixture of coal-tar and plaster.

Nitrate of lead,* *Creosote*, and other substances which have been proposed at one time or another, have not realized the expectations of their inventors; their price has been too great, their employment required too much care, or their action has not been sufficiently certain that they could be advantageously used in practice. There is, nevertheless, one of these which deserves special mention, viz., *chlorine*. Ever since Guyton Morveau demonstrated the true action of muriatic acid upon putrefying animal matters, the efficacy of chlorine has been tested in almost innumerable ways. Solutions of chlorine, of "chlorid of soda," and of "chlorid of lime," have rendered signal services to medicine and in the cause of public

ous, and muriatic acids; peroxyd of iron, highly dried clay, litharge, and saw-dust were found imperfect even when very large quantities were employed.

Arsenious acid and creosote on the contrary, were very active; but the danger of a subsequent evolution of arseniuretted hydrogen in the first case, and the difficulty of diffusing an oily fluid like creosote in the second, seemed to interdict the use of these substances. F. H. S.

* [An excellent, though somewhat expensive "disinfecting fluid" (Ledoyen's), which was quite extensively used in this country a few years since, consisted, according to analyses of F. E. Holyoke, of an aqueous solution of this salt.—F. H. S.]

health, especially since Labarraque, some thirty years since, indicated an improved method of employing them. But the odor of chlorine, disagreeable in itself, is neither easily borne nor devoid of inconveniences. Wounds, moreover, hardly accommodate themselves to it any better than the sense of smell, whenever somewhat large doses of it are required.

Chlorinated Sponge.—The idea of applying sponges saturated with chlorinated solutions, directly upon purulent or gangrenous wounds, as suggested by Hervieux, appears to be excellent for certain cases. Such sponges, renewed several times per day, absorb the pus gradually as it forms better than anything else, and disinfect the wound very well. Unfortunately, chlorine rapidly alters or destroys the sponges and soon causes undue irritation. While this method, therefore, is an excellent one for cleaning certain gangrenous and sinuous wounds, it is, nevertheless, inferior in most instances to the mixture of coal-tar and plaster.

Subnitrate of bismuth—suggested by Frémy as an absorbent and disinfectant, was applied to a large number of wounds. Upon large cavernous cancers it disinfected somewhat better than Peruvian bark, charcoal, or chlorate of potash, but less than the coal-tar and plaster. By its use, however, several bad looking wounds were cleansed quite rapidly. Since it causes no pain or irritation, and since it neither soils the skin nor the clothes, the subnitrate of bismuth is preferable to a multitude of other antiseptic powders; but it is useful rather as a solidifier (*incarnatif*), or dryer, than as an absorbent or disinfectant.

In their *résumé* the committee affirm:

I. That coal-tar mixed with plaster, according to the formula of Corne, (see this Journal, xxviii, 426), can disinfect putrefying organic matters. Mixed with alvine dejections this powder destroys their odor, and leads one to hope that by its aid profound reforms in the present system of maintaining and clearing out cess-pools, &c., may some day be brought about. For this purpose, ordinary earth, coal-ashes, or sand may be substituted for the plaster, as Cabanes prefers, being at least equally efficacious.

II. In therapeutics the coal-tar and plaster has fulfilled only a part of its promises. As a disinfectant in the dissecting-room, upon the folds of bandages,—everywhere where there is infectious matter, its qualities are incontestable. This is also true as regards putrid or gangrenous foci, fetid suppurations, sanious wounds, ichorous putrilagenous cavities, hospital gangrene, &c.; but upon acute and exposed wounds, or upon ordinary wounds or ulcers, other topical applications are preferable to it.

III. Used with lint upon cloths, with pomades or cerate, it has afforded no useful result, and nothing has occurred to prove that when administered internally it has produced the least benefit.

IV. As an absorbent it leaves much to be desired, although it is not entirely devoid of action. When applied as a poultice, in particular, it absorbs very incompletely. For that matter the mixtures of coal-tar with earth or with other powders, absorb still less than the mixture of Corne and Demeaux, and are scarcely at all applicable in therapeutics. In this connection it must not be forgotten that the morbid liquids, and particularly pus, are very different from water. A substance like plaster, for example, which absorbs water strongly, might be incapable of absorbing pus. It is nevertheless true, that as an absorbent, the mixture of coal-

tar and plaster, either as powder or as poultice, is of some use upon fetid and putrid wounds or suppurations.

V. Cellulose, glycerine and cherry-laurel water; chlorate of potash mixed with talc, clay, marl or kaolin, are neither sufficiently efficacious, nor in application are they convenient enough to be retained in practice.

VI. The mixture of saponine and coal-tar does not appear to be preferable for dressing wounds to many other liquids already known,—tincture of aloes for example. The same may be said of the mixed coal-tar and charcoal of Herpin; nor does it seem as if carbonic acid should be used, unless some improved method of applying it can be devised.

VII. The Boghead residue would be useful only in lack of coal-tar and plaster. While charcoal in porous envelopes does not mould itself to cavernous and sinuous wounds with sufficient readiness to come into general practice.

VIII. From its low price, and by its action, at once mild, absorbent, and disinfectant, as well as by its drying properties, the subnitrate of bismuth will render important services in default of the mixture of coal-tar and plaster. It is even preferable to this when the wounds are accompanied or surrounded with heat or irritation.

IX. Tincture of iodine and perchlorid of iron act rather by modifying the surfaces of wounds and of purulent foci, than as absorbents or disinfectants. They have their special applications in surgery, but agents of this sort are not comparable with the mixed coal-tar and plaster.

X. Sponges soaked in chlorinated water can also render good service upon pale, burrowing sores and upon gangrenous foci.

We have occupied ourselves, say the committee, only with the practical or experimental side of the question. A discussion of its theoretical or chemical bearings would have carried us too far. Moreover, since the authors of the different communications which have been submitted to us have themselves neglected this for the most part, it has seemed to us useless to treat of it at present; whether it be the phenic acid or rosolic, or brunolic acid, or the anilin, picolin, etc., of the coal-tar, which disinfects, is in reality of but little importance. Science will inform us of this some day no doubt; for the moment we have merely to ascertain whether or no the various disinfectants which are brought to us do really disinfect.

After citing the labors of various persons who have proposed methods of disinfection, the committee go on to say: "M. Corne, and the authors indicated above, occupied themselves only with the disinfection and the solidification of animal matter, having in view the preparation of manures. * * * * It is M. Demeaux who appears first to have had the thought of applying to fetid wounds, in surgical practice, the powder invented, or adopted and extolled, by his neighbor. In addition, it is evident that here, as is the case with so many other complex facts with which science is enriched, there is, so to say, neither invention nor priority for any one. The subject has been worked upon for more than a century—a multitude of savans having competed with each other in studying it. Little by little the evolution of the discovery has been effected. M. Corne disengaged it from its gangue a little better than his predecessors, and Demeaux, knowing, perhaps, that from time immemorial sailors and the inhabitants of certain southern countries often dress their wounds

with tar; that tar water and pomades of tar are frequently employed in medicine, has extended its applications to therapeutics."

"Many other efforts are still necessary. In point of fact the results thus far obtained are merely rough outlines,—only first trials. So long as the world at large is not in possession of a simple, easy, and economical method, accessible to every one, which shall be capable of disinfecting immediately, and without inconvenience on the large or small scale, dejections and filth of all kinds, in dwellings as well as in privies or slaughter-houses; in dissecting-rooms and the like, as well as in the sick room, upon wounds, improvements will still be wanted; there will yet be room for new attempts. While recording those of to-day and those of yesterday upon the road already traversed, let us be careful not to diminish the ardor of the laborers in the future, who will finally endow civilization with a complete and general disinfection."

Finally, certain indispensable precautions must be followed, in order to obtain from the process of Corne and Demeaux its proper effect. It is evident, from having neglected some of these precautions, that different experimenters have been led to believe that the method is useless. Fine moulding plaster, and not the common article, should be employed. The coal-tar, which is mixed with it in the proportion of 2 to 4 parts to a hundred, by triturating or grinding, ought to impart to it a gray tint, without destroying its dry, pulverulent condition. Objects to be disinfected should be rolled in this powder until each point upon their surfaces has been brought in contact with it. Gangrenous or putrid sores should be covered with thick layers of it; by handfuls, several times per day. If one is treating pus, blood, dejections, or the like, enough of the powder should be added to form a paste of the mass, taking care to replace the first layer of powder by another as soon as it no longer absorbs any more. Mixed with oil to the consistence of a thick pap, it forms poultices of convenient application, if they are made thick and broad.

Within the limits which have been indicated the mixture of coal-tar and plaster is a good disinfectant, and may be recommended for use in domestic economy as well as in hospitals. "What we have ourselves seen leaves no uncertainty of the reality of this property, nor of the possibility of its application." * * * * * It remains only to draw from it reasonable, practical consequences, either taking the fact as it is, or by modifying and perfecting it.—*Comptes Rendus*, l, 279.

[For corroborating testimony, received by the committee from various sources, the reader is referred to their report in question. Numerous other articles upon the subject, by different authors, may also be found in vol. xlix of the *Comptes Rendus*.—F. H. S.]

7. *Decoloration of Indigo by Sesquioxyd of Iron.*—[In the May number of this Journal we took occasion to maintain that the very interesting fact of the power of ferric salts to bleach solutions of indigo was first observed by Prof. H. Wurtz. Since then we have accidentally learned that this claim was erroneous, as will appear from the following statement made by Wœhler some twenty years since. "When a solution of indigo in sulphuric acid is mixed with salts of the sesquioxyd of iron and heated, it will be decolorized precisely as it would be by nitric acid."—(Annalen der Chemie und Pharmacie, 1840, xxxiv, 235; see also Gmelin's Handbook (Cavendish Soc. Edit.), xiii, 59.—F. H. S.]

II. GEOLOGY.

1. *On Some Points in Chemical Geology;* by T. Sterry Hunt, F. R. S. (Read before the *Geol. Society of London*, January 5th, 1859--published in the *Quarterly Journal* of the Society for November, 1859—and reprinted, with additional notes by the author, in the *Canadian Naturalist* for January, 1860.).—In this paper the author discusses a number of questions which lie at the foundation of a true history of the chemistry of the earth's crust, and gives farther developments to some of his peculiar views, which were for the most part, first enunciated in this Journal. In regard to the metamorphism of sedimentary deposits, *i. e.*, the conversion of sands, clays and marls into crystalline stratified rocks, the author, after distinguishing between local and normal metamorphism, insists upon the frequent interstratification of unchanged fossiliferous limestones among crystalline schists as evidence that heat has not been the only agent in the metamorphism, which has moreover been effected at temperatures not very elevated, and by the intervention of alkaline solutions, in the absence of which, sediments may be heated to the same degree without change.

The first announcement of this view will be found in this Journal for May, 1857 (vol. xxiii, p. 407), where the author, after describing some experiments with the alkaline silicates, expresses the opinion that "we have here the explanation of rock metamorphism in general." Farther inquiries into the action of the soluble alkaline silicates will be found in this Journal for March and May, 1858 (vol. xxv, pp. 287–437), where the subsequent experiments of Daubrée are cited in confirmation and extension of Mr. Hunt's theory of the normal metamorphism of sediments at comparatively low temperatures by the intervention of alkaline carbonates and silicates, which may be either liberated by the decomposition of the sediments themselves or derived from adjacent strata. These salts in solution permit the crystallization of feldspars and micas, or when alkaline bases are present only in smaller quantity, of kyanite, andalusite and staurotide, while by the intervention of protoxyd bases, garnet, epidote, chloritoid and chlorite are formed, and in the absence of the argillaceous element, pyroxene, olivine, serpentine and talc. In a subsequent note the author has however alluded to the probable direct formation of certain silicates of magnesia and lime, in open basins at the earth's surface and by reactions at the ordinary temperature. This Journal, March, 1860 (xxix, 284).

In the second place the author discusses the relations of plutonic to metamorphic sedimentary rocks, and concludes that the latter, becoming plastic under the influence of water and heat, may be displaced by disturbance and pressure, thus taking the form of intrusive rocks. Sediments altered *in situ* he distinguishes as indigenous, and those displaced as exotic plutonic rocks. The conclusions of Scrope, Scheerer and Elie de Beaumont, supported by the late observations of Daubrée and Sorby, as to the aqueo-igneous fusion of these rocks, are fully admitted.

In the third place the author discusses the theories of Phillips, Bunsen and Durocher, as to the origin of intrusive rocks, and rejecting the notion that these are derived from the supposed fluid interior of the earth, re-

gards them as in all cases, fused and displaced sediments. He proceeds to show that the action of waters removing from permeable strata their soda, lime and magnesia, brings these to the composition of granitic rocks, while the finer and less permeable sediments, retaining their protoxyd bases, give rise by subsequent alterations to basic rocks with triclinic feldspars and pyroxene. Soda being preëminently the soluble alkali, has been gradually removed from disintegrated feldspathic rocks under the influence of water and carbonic acid, and the carbonate of soda thus formed has by its reaction with the lime salts of the ancient ocean, given rise to sea-salt and to the carbonate of lime with which the limestones have been built up. The aluminous silicate set free in the decomposition of the feldspars is thus the equivalent of the earthy carbonates and sea-salt which are formed. Hence we find that in the oldest known crystalline rocks, those of the Laurentian series, soda feldspars are abundant, micaceous schists rare, and argillites or silicates of alumina deficient in alkali are unknown, while in higher formations, argillites and schists with kyanite, chiastolite and staurotide abound, as well as chlorite, chloritoid, muscovite, garnet, epidote, etc., showing a great excess of aluminous silicate over the alkali required to form feldspars.

These views have already appeared in a communication from the author in this Journal (vol. xxv, p. 436) where also the action of organic matters in deoxydizing, dissolving and removing oxyd of iron from certain strata to be accumulated in others, is discussed and illustrated by a consideration of the iron deposits of various ages. The existence of beds of iron ore in the Laurentian rocks, not less than the graphite and metallic sulphurets which these contain, is by the author regarded as evidence that organic life existed at the period when these rocks were deposited. As Mr. Hunt has elsewhere explained, he supposes the condition of the cooling globe to have been one of thorough oxydation, and regards all processes of reduction or deoxydation as dependent either directly or indirectly upon organic life. In regard to iron oxyd however he remarks that its solution may sometimes be due to mineral acids, from volcanic or other sources; such solutions, and others from the oxydation of pyrites, may be decomposed by alkaline or earthy carbonates and give rise to iron deposits. A similar process with aluminous solutions will serve to explain the origin of corundum, beds of emery, and aluminous iron ores.

In regard to the agency of organic matters in the formation of iron deposits we may here remark that a reviewer in this Journal (xxv, 245) in noticing Mr. Hunt's observations on this subject contained in his Geological Report for 1856, writes as if Mr. H. had appropriated the views of Bischof. In truth neither Bischof nor Hunt ever claimed any originality in bringing forward a principle which has long been understood, and which they have only attempted to extend and develope. We may here observe that the same reviewer fails to apprehend Mr. Hunt's views on the formation of crystalline rocks, when he says that Bischof and Hunt agree in supposing all rocks to be formed by chemical agencies in the presence of water, and that therefore the latter cannot claim originality. Now upon the point in question there is little or no affinity between the views of the two writers, for the simple reason that Bischof seems never for a moment to apprehend the nature of the great problem which Mr. Hunt has under-

taken to solve, but with Dana regards normal metamorphism as pseudomorphism on a grand scale. The ingenious speculations of Bischof and others on the possible alteration of mineral species by the action of various saline and alkaline solutions may pass for what they are worth, although we are satisfied that by far the greater part of the so-called cases of pseudomorphism in silicates are purely imaginary, and when real are but local and accidental phenomena. Bischof's notion of the pseudomorphism of silicates like feldspars and pyroxene presupposes the existence of crystalline rocks, whose generation this neptunist never attempts to explain, but takes his starting point from a plutonic basis.

The problem to be solved, as we have elsewhere insisted, is the conversion of sands, clays and marls, (consisting of silica, silicates of alumina, carbonates of lime, magnesia and oxyd of iron, derived by chemical and mechanical agencies from the ocean waters and pre-existing crystalline rocks,) into aggregations of crystalline silicates. These metamorphic rocks, once formed, are liable to alteration only by local and superficial agencies, and are not, like the tissues of a living organism, subject to incessant transformations, the pseudomorphism of Bischof and Dana. As yet, Mr. Hunt is the only one who has attempted a rational explanation, based on experiments, of the problem of the conversion of sedimentary strata into crystalline rocks, and his views, whether true or false, are to be judged by themselves, and not by comparison with those of Bischof or any other writer. Among the geologists who since the time of Hutton, have best comprehended the nature of the problem of rock metamorphism, are Boué, Virlet and Delanouë. We hope at an early day to discuss in the pages of this Journal, the question of mineral pseudomorphism, as well as the history and theory of metamorphism.

Passing from plutonic to volcanic phenomena, Mr. Hunt proceeds to develope the views of Babbage and Herschel as to the effects of the internal heat upon deeply buried sediments. Babbage has shown that the expansion of the sedimentary rocks by heat may cause great vertical movements, while in the subsequent fusion of the heated sediments Herschel finds an explanation of volcanic phenomena. These views the author adopts, and enters into a consideration of the relations which must take place between silica and silicates, carbonates, sulphates, chlorids and organic matters in the presence of water at an elevated temperature. To azotized organic substances and to the ammonia condensed in argillaceous strata, he ascribes, with Bischof, the ammoniacal salts of volcanoes.

In considering the metamorphism of the strata, which must always precede volcanic action, we are not to lose sight of a process which will, in its results, be the reverse of that insisted upon by Babbage. It is the great contraction which must result, not only from the solidification of the porous sediments, but from the condensation attendant upon chemical combination, by which they are converted into silicates of high specific gravity, such as pyroxene, garnet, epidote, chloritoid and chiastolite. In this way, as remarked by Mr. Hunt in his lectures before the Smithsonian Institution at Washington last winter, we may realize, to a certain extent, Elie de Beaumont's notion of a shrinking of the earth's nucleus, and find an explanation of many phenomena of subsidence and corrugation, although with Herschel we are to attribute these for the most part to "the disturb-

ance of the equilibrium of pressure consequent upon the transfer of sediments while the yielding surface reposes upon a mass of matter partly liquid and partly solid."

We conclude with the following extracts, and with two notes appended to the Canadian reprint of Mr. Hunt's papers, in one of which he calls attention to the remarkable work of Keferstein, whose ingenious views, too much in advance of his time, have hitherto been overlooked.

"The metamorphism of sediments *in situ*, their displacement in a pasty condition from igneo-aqueous fusion as plutonic rocks, and their ejection as lavas with attendant gases and vapors are then all results of the same cause, and depend upon the differences in the chemical composition of the sediments, the temperature, and the depth to which they are buried: while the unstratified nucleus of the earth, which is doubtless anhydrous, and according to the calculations of Messrs. Hopkins and Hennessey, probably solid to a great depth, intervenes in the phenomena under consideration only as a source of heat." *

"The volcanic phenomena of the present day appear, so far as I am aware, to be confined to regions covered by the more recent secondary and tertiary deposits, which we may suppose the central heat to be still penetrating (as shown by Mr. Babbage), a process which has long since ceased in the palæozoic regions. Both normal metamorphism and volcanic action are generally connected with elevations and foldings of the earth's crust, all of which phenomena we conceive to have a common cause, and to depend upon the accumulation of sediments and the subsidence consequent thereon, as maintained by Mr. James Hall in his theory of mountains. The mechanical deposits of great thickness are made up of coarse and heavy sediments, and by their alteration yield hard and resisting rocks; so that subsequent elevation and denudation will expose these contorted and altered strata in the form of mountain chains. Thus the Appalachians of North America mark the direction and extent of the great accumulation of sediments by the oceanic currents during the whole

* "The notion that volcanic phenomena have their seat in the sedimentary formations of the earth's crust, and are dependent upon the combustion of organic matters, is, as Humboldt remarks, one which belongs to the infancy of geognosy. In 1834 Christian Keferstein published his *Naturgeschichte des Erdkörpers*, in which he maintains that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena according to him have their origin, not in an igneous fluid centre, nor an oxydizing metallic nucleus, but in known sedimentary formations, where they are the result of a peculiar process of fermentation, which crystallizes and arranges in new forms the elements of the sedimentary strata, with evolution of heat as an accompaniment of the chemical process. (*Naturgeschichte*, vol. i, p. 109, also *Bull. Soc. Géol. de France* (1) vol. vii, p. 197).

These remarkable conclusions were unknown to me at the time of writing this paper, and seem indeed to have been entirely overlooked by geological writers; they are, as will be seen, in many respects an anticipation of the views of Herschel and my own; although in rejecting the influence of an incandescent nucleus as a source of heat, he has, as I conceive, excluded the exciting cause of that chemical change, which he has not inaptly described as a process of fermentation, and which is the source of all volcanic and plutonic phenomena. See in this connection my paper *On the Theory of Igneous Rocks and Volcanoes*, in the Canadian Journal for May, 1858."

palæozoic period; and the upper portions of these having been removed by subsequent denudation, we find the inferior members of the series transformed into crystalline stratified rocks."*

III. BOTANY AND ZOOLOGY.

1. *Flora of the Southern United States, containing abridged descriptions of the Flowering Plants and Ferns of Tennessee, North and South Carolina, Georgia, Alabama, Mississippi, and Florida; arranged according to the Natural System;* by A. W. CHAPMAN, M.D. (The Ferns by Daniel C. Eaton.) New York: Ivison, Phinney & Co. 1860. pp. 621, small 8vo.—The first thing that strikes our attention as we open this volume, is its neat and tasteful typography. It is a decided advance upon its counterpart, Gray's Manual for the Northern States, in this respect, and indeed is the handsomest volume of the kind we know of. It is only just to add that the book was produced by the University Press of Welch, Bigelow & Co., Cambridge. The matter of the volume is, we trust, as good as its form. It well supplies a long-felt and pressing want, and gives to schools and colleges, and to botanical students generally at the South, a work which is for that district what Gray's Manual is for the northern section of our common country. Having said this, modesty prevents more particular eulogium. If experience annually shows that the work with which this volume is compared is not yet perfect, but still requires many minor emendations, notwithstanding long pains-taking and

* "The theory that volcanic mountains have been formed by a sudden local elevation or tumefaction of previously horizontal deposits of lava and other volcanic rocks, in opposition to the view of the older geologists who supposed them to have been built up by the accumulation of successive eruptions, although supported by Humboldt, Von Buch, and Elie de Beaumont, has been from the first opposed by Cordier, Constant Prevost, Scrope and Lyell. (See Scrope, *Geol. Journal*, vol. xii, p. 326, and vol. xv, p. 500; also Lyell, *Philos. Trans.*, part 2, vol. cxlviii, p. 703. for 1858.) In these we think will be found a thorough refutation of the elevation hypothesis and a vindication of the ancient theory.

This notion of paroxysmal upheaval once admitted for volcanoes was next applied to mountains which, like the Alps and Pyrenees, are composed of neptunian strata. Against this view, however, we find De Montlosier in 1832 maintaining that such mountains are to be regarded as the remnants of former continents which have been cut away by denudation, and that the inversions and disturbances often met with in the structure of mountains are to be regarded only as local accidents. (*Bul. Soc. Geol.*, (1) vol. ii p. 438, vol. iii, p. 215.)

Similar views were developed by Prof. James Hall in his address before the American Association for the Advancement of Science, at Montreal in August, 1858. This address has not been published, but these views are reproduced in the first volume of his *Report on the Geology of Iowa*, p. 41. Mr. Hall there insists upon the conditions which in the ancient seas, gave rise to great accumulations of sediment along certain lines, and asserts that to this great thickness of strata, whether horizontal or inclined, we are to ascribe the mountainous features of northeastern America as compared with the Mississippi valley. Mountain heights are due to original deposition and subsequent continental elevation, and not to local upheavals or foldings, which on the contrary, give rise to lines of weakness, and favor erosion, so that the lower rocks become exposed in anticlinal valleys, while the intermediate mountains are found to be capped with newer strata.

In like manner J. P. Lesley asserts that 'mountains are but fragments of the upper layers of the earth's crust,' lying in synclinals and preserved from the general denudation and translation. (*Iron Manufacturer's Guide*, 1859, p. 53) See also his admirable little volume entitled *Coal and its Topography*."

repeated revisions, it may be expected that equal experience will reveal similar imperfections in the new and untried work. None but a practised teacher can tell beforehand where the pupil, or the student without a teacher will encounter obstacles, and the most experienced can only partially anticipate them. They must be found out by trial, and be corrected in new issues,—for which electrotyping offers great facilities. For the young student the Artificial Analysis of the Natural Orders is practically the key to the whole thing; a perfect key of this sort was never made at one trial; in fact most keys in such works fail very largely. So we may safely hazard the prediction that Dr. Chapman's artificial analysis will need emendation in a future edition. We are bound to add, however, that a half dozen of trials has resulted in only one failure.

This book is wanted by botanists as well as by students, and we think they will be well pleased with it. Its merits are manifold; its deficiencies are either such as are natural if not unavoidable under the circumstances of its production, or such as pertain to all works of the kind, in which knowledge is progressive, the records of this knowledge widely dispersed, and the facts to be observed and digested into order almost infinitely numerous. An introduction gives a good condensed sketch of the Elements of Botany, and a Glossary of Botanical Terms, so that the book can be used independently; though such a book as the First Lessons in Botany ought to precede and accompany its use by the student. Very appropriately is this volume dedicated to one of the worthiest botanists of the Southern States, the Rev. Dr. Curtis. Now that the Southern Atlantic states are provided with a good Flora of their own, we trust that botany will receive a new impulse, both as a scientific pursuit and as a branch of education, in that favored region. A. G.

2. *Synopsis of Dalbergieæ, a Tribe of Leguminosæ;* by George Bentham, Esq.—This is a (botanical) supplement to the fourth volume of the Journal of the Proceedings of the Linnæan Society, a critical account of this group of plants, and a technical synopsis of the genera and species,—of much importance therefore to the systematic botanist. Since its publication, better specimens have been examined of one of Fendler's Venezuelan plants, No. 2223, referred to on p. 17; and Mr. Bentham finds this a new generic type, *Fissicalyx.* A. G.

3. *Reports on the Natural History, Climate, and Physical Geography of Minnesota, Nebraska, Washington, and Oregon Territories; made in connection with the Survey of a Pacific Railroad Route, along the forty-seventh and forty-ninth parallels of latitude, in* 1853–4–5–6, *under the command of Governor* I. I. Stevens, *of Washington Territory;* by G. Suckley, M.D., and J. G. Cooper, M.D., Surgeons and Naturalists to the Expedition. 4to, pp. 399, 26, with 65 plates, and isothermal charts, &c. New York, Bailliere Brothers.—This is a separate issue of the 12th volume of the Pacific Railroad Exploration Reports, containing all that relates to science in the report of Gov. Stevens' survey of the northern proposed route, and also a preface and other additional matter by the enterprising authors, Drs. Cooper and Suckley. This issue is, we understand, rendered necessary by the cutting down of the government edition in the later volumes of the series to a much diminished number. The authors have taken this opportunity not merely to add new matter, and

make certain emendations, but also to insert four or five pages of errata, correcting typographical errors, perhaps a tithe of those contained in the volume. This remark does not at all militate against the statement that in this volume "there are fewer errors than is usual in similar government publications." We are satisfied of the truth of this. The mass of scientific reports published by Congress and printed at Washington are marred beyond all endurance by want of proof-reading, or perhaps of proof-correction,—for in some instances proofs were furnished and revised, but the corrections were never made. The folly of the late system having now been exposed in other and more considerable respects, we may hope for the inauguration of a much better and more economical plan.

The first chapter of the present very interesting volume (whether peculiar to this edition or no is more than we can now ascertain) is separately paged, and is devoted to the meteorology and climate of Nebraska and Washington Territories. The Botanical Report, which comes next in order, consists, 1. of a very interesting and useful general sketch of the botany of the route, in reference to the character of the vegetation, geographical distribution, &c., mingled with zoological and climatal observations. 2. A catalogue of the plants collected east of the Rocky Mountains, containing three before undescribed species and one new (Chenopodiaceous) genus,—all of which are characterized by Dr. Torrey, as will be seen, although the list was drawn up by Dr. Gray. This portion is illustrated by four excellent plates, one of which is devoted to the *Endolepis Suckleyi* of Torrey, the new genus referred to. 3. Catalogue of Plants collected in Washington Territory; with observations, &c., drawn up by Dr. Cooper himself, including notes, characters, &c., supplied by Dr. Gray, Dr. Torrey, and Prof. Thurber; with two plates.

The remainder and most striking part of the volume is the Zoological Report; that on the Insects by Dr. Leconte; on the Mammals by Dr. Cooper, Dr. Suckley and Mr. Gibbs; on the Birds, by Drs. Cooper and Suckley; on the Reptiles, by Dr. Cooper; on the Fishes by Dr. Suckley; on the Mollusca by Wm. Cooper, Esq. (a veteran naturalist whom we gladly welcome back to active labor); on the Crustacea by Dr. Cooper. The zoological plates are many and truly beautiful; those of the birds are colored. The volume sells for ten dollars; and this small separate issue will doubtless be taken up at once, to complete the sets of the Pacific Railroad Reports. A. G.

4. *Potamogeton crispus* L. was introduced into the North American flora by Pursh, and said to occur from "Canada to Virginia." Dr. Torrey, in his Flora of the Northern States, mentions it as from Lake George; but as he omits it from his recent Flora of the State of New York, we infer that there was some mistake in the first instance. Prof. Tuckerman, who has paid great attention to this difficult genus, not having found *P. crispus* in this country, and not having ourselves met with it, the species was excluded from the Manual of the Botany of the Northern United States; but a remark was added in the second edition of this work, that Mr. Tuckerman had seen a specimen in some European herbarium purporting to come from Delaware. It may also be noted that this species has for many years been growing in a pool in the Botanic Garden at Cambridge,—where in fact it cannot be got rid of,—and there

is a tradition that it was introduced into the pool by Mr. Nuttall. Last year Mr Edward Tatnall, an intelligent botanist and horticulturist, detected the plant in question in Delaware, in the vicinity of Wilmington, and this season he finds it to occur abundantly, under circumstances which give it a good claim to be regarded as indigenous. So *Potamogeton crispus* must be restored to our flora. If really indigenous it probably occurs in other stations. Its early flowering and fruiting, compared with the other species (viz., blossoming in May) may have led to its being overlooked; but the species is probably local in this country. It is, however, so vigorous and so difficult of eradication where it is established, so likely therefore to hold its own or to extend, that, if not detected elsewhere, we may believe that it was recently imported into this country, as another water-weed, the *Anacharis* of North America, was into England, where it has spread prodigiously with a few years. A. G.

5. *Marsilea quadrifolia*, L.—Aquatic plants, especially those of low type, are in general so widely diffused geographically, that the absence from North America of the above named plant,—so common throughout the northern part of the Old World,—has always seemed rather exceptional. We have now to announce its actual occurrence here. It has just been discovered on the muddy borders of a pond in Litchfield, Connecticut, by Dr. Timothy F. Allen. This adds another instance of the apparently extremely local occurrence in this country of a common European species, of which *Scolopendrium officinarum* and *Subularia aquatica* are cases in point. As it is not likely that this Marsilea was created for Litchfield pond, or for any other few localities, if such there be, in this country, such plants must be regarded as of recent and casual introduction—which is most improbable—or else as species once diffused over the country, but now on the verge of extinction from this flora,—a view which chimes in with other inferences about geographical distribution.

A. G.

6. *Catalogue of the Acanthopterygian Fishes in the collection of the British Museum;* by Dr. A. Gunther. 8vo, pp. 524. London, 1859. —In this volume all the fishes in the British Museum, belonging to the families mentioned are described, and abstracts are given of the characters of many others, not in that collection. The work must be therefore, useful to naturalists, and especially to students, as the author has been quite diligent in collecting the indications of species from all sources. Considerable discretion must however be used in consulting it, as the author appears to have considered many species that he had not himself seen, as being very "doubtful" or as identical with some known to him. In his work, our American fresh-water fishes are especially in great disorder. None of the genera recently established by Agassiz and Girard are adopted. The *Calliuri* are placed partly in *Centrarchus* and partly in *Brythus*. The *Grystes* are distributed in *Centrarchus* and *Huro* as well as *Grystes*. If recent American works had been consulted these errors would not have occurred. Some species are regarded as identical which have no close relationship to each other, as *Pomotis falax*, B. and S., and *P. rubricanda* Storer. In many other points Dr. Gunther differs from the best Ichthyologists who have hitherto treated of the order. Thirteen genera and forty species are described as new. W. S.

IV. ASTRONOMY.

1. *First Comet of* 1860.—(Gould's Astron. Jour. No. 134).—"A letter from Mr. Liais, Director of the Brazilian Coast Survey, to Prof. Peters, published in the *Astronomische Nachrichten*, No. 1248, announces the discovery, February 26th, at Olinda, Brazil, of a faint double comet, near the star μ Doradûs. The larger portion preceded; it was brighter on the side toward the sun, and sensibly elongated in that direction, having near the extremity a small luminous point, about as bright as a star of the 9th magnitude. The object was so faint as to render observations difficult, for which reason the diameter could not be measured; but Mr. Liais estimated it at 25 or 30 seconds in the larger, and 7 or 8 seconds in the smaller diameter. The second or smaller nebulosity appeared nearly circular, and about 4 seconds in diameter. On February 27th, at $10^h\ 25^m$ it followed the other by about 27 seconds, being about $1'\ 8''$ to the North of it. On March 3d, at $11^h\ 16^m$, the difference of position was 23 seconds in right ascension, and $46''$ in declination. On the 6th, the moonlight wholly extinguished the comet.

From observations made by Mr. Liais with the ring-micrometer, Dr. Pape, of Altona, has computed three normal places, and thence deduced elements as follows, viz.:—

Time of perihelion passage, Febr. 16·7667, Berlin m. t.

Long. of perihelion, - -	173° 26′ 2	App. eqx.
" " asc. node, - - -	324 1 9	Febr. 29·6
Inclination, - - - - -	79 22 6	
Log. perih. dist., - - - -	0·07652	
Motion, - - - -	Direct.	

2. *Second Comet of* 1860.—On the 17th of April, 1860, a telescopic comet was discovered by Mr. George Rümker, of Hamburg. It was a faint, ill-defined nebulous spot of light. Having passed its perihelion at the close of the preceding February, it was when discovered receding from the sun. Its elements, approximately determined, are found to resemble those of the second comet of 1793.

3. *On the alleged intra-Mercurial Planet.*—According to the elements assigned by M. Leverrier to the planet which Dr. Lescarbault states that he observed passing across the sun March 26, 1859, there was reason to expect that the planet would be seen in like transit, sometime in March or April of the present year. In the hope of detecting such a transit the sun's disc was closely watched during these months by observers in three places at least in this country, and doubtless also by many observers in Europe. So far as we hear, the search has everywhere proved unsuccessful. But as during this period there were many hours in which the sun's disk was not and could not be under observation, the failure does not disprove the existence of such a planet. It is to be hoped that the search will be resumed hereafter, and as there is great uncertainty respecting the inclination of the planet, a thorough observation at any time, with a magnifying power of 100 or more will be valuable. If the spot seen on the sun, February 12, 1820, by Steinhübel and by Stark, (Mon. Not. Roy. Astr. Soc., March 9, 1860,) was this planet, the inclination must be quite small, and a transit across the sun may often occur.

Lescarbault's alleged observation of March 26, 1859, is contradicted by M. Liais, in No. 1248 of the Astronomische Nachrichten. The latter states that on that day he was observing, with a view of determining the relative intensities of the different parts of the disc, at San Domingos, in the Bay of Rio Janeiro, from $11^h\ 4^m$ to $11^h\ 20^m$, and from $12^h\ 42^m$ to $1^h\ 17^m$ S. Dom. mean solar time. Allowing three hours for difference of longitude, the ingress of the spot should have been at $1^h\ 5^m$ at St. Domingo. At $1^h\ 17^m$ the spot should accordingly have been on the sun's disc for twelve minutes and would in that time have traversed an arc of $1'\ 4$. M. Liais asserts he must have seen such a spot had it been there. He shows further that such a planet as the one supposed would be easily seen during a large part of its orbit, by the telescope, and often by the naked eye of an observer within the tropics. As the planet has not been thus seen he does not believe it to exist.

V. BOOK NOTICES.

Prof. Agassiz on the Origin of Species.

1. *Contributions to the Natural History of the United States;* by L. Agassiz.—The third volume of this work, now in the press, will appear shortly. We copy from the advance sheets the following paragraphs relating to the origin of species, which has lately attracted much attention, in consequence of the publication of Darwin's book on that subject.

Individuality and Specific Differences among Acalephs.

The morphological phenomena discussed in the preceding section naturally lead to a consideration of individuality and of the extent and importance of specific differences among the Acalephs. A few years ago the prevailing opinion among naturalists was that, while genera, families, orders, classes, and any other more or less comprehensive divisions among animals were artificial devices of science to facilitate our studies, species alone had a real existence in nature. Whether the views I have presented in the first volume of this work (p. 163), where I showed that species do not exist in any different sense from genera, families, etc., have had any thing to do with the change which seems to have been brought about upon this point among scientific men, is not for me to say; but whatever be the cause, it is certainly true that, at the present day, the number of naturalists who deny the real existence of species is greatly increased. Darwin in his recent work on the "Origin of Species," has also done much to shake the belief in the real existence of species, but the views he advocates are entirely at variance with those I have attempted to establish. For many years past I have lost no opportunity of urging the idea that while species have no material existence, they yet exist as categories of thought, in the same way as genera, families, orders, classes, and branches of the animal kingdom. Darwin's fundamental idea, on the contrary, is that species, genera, families, orders, classes, and any other kind of more or less comprehensive divisions among animals do not exist at all, and are altogether artificial, differing from one another only in degree, all having originated from a successive differentiation of a primordial organic form, undergoing successively such changes as would

at first produce a variety of species; then genera, as the difference became more extensive and deeper; then families, as the gap widened still farther between the groups, until in the end all that diversity was produced which has existed or exists now. Far from agreeing with these views, I have, on the contrary, taken the ground that all the natural divisions in the animal kingdom are primarily distinct, founded upon different categories of characters, and that all exist in the same way, that is, as categories of thought, embodied in individual living forms. I have attempted to show that branches in the animal kingdom are founded upon different plans of structure, and for that very reason have embraced from the beginning representatives between which there could be no community of origin; that classes are founded upon different modes of execution of these plans, and therefore they also embrace representatives which could have no community of origin; that orders represent the different degrees of complication in the mode of execution of each class, and therefore embrace representatives which could not have a community of origin any more than the members of different classes or branches; that families are founded upon different patterns of form, and embrace representatives equally independent in their origin; that genera are founded upon ultimate peculiarities of structure, embracing representatives, which, from the very nature of their peculiarities could have no community of origin; and that finally, species are based upon relations and proportions that exclude, as much as all the preceding distinctions, the idea of a common descent.

As the community of characters among the beings belonging to these different categories arises from the intellectual connection which shows them to be categories of thought, they cannot be the result of a gradual material differentiation of the objects themselves. The argument on which these views are founded may be summed up in the following few words: Species, genera, families, &c. exist as thoughts, individuals as facts. It is presented at full length in the first volume of this work, (p. 137–168), where I have shown that individuals alone have a definite material existence, and that they are, for the time being, the bearers not only of specific characteristics, but of all the natural features in which animal life is displayed in all its diversity; individuality being, in fact, the great mystery of organic life.

Since the arguments presented by Darwin in favor of a universal derivation from one primary form, of all the peculiarities existing now among living beings have not made the slightest impression on my mind, nor modified in any way the views I have already propounded, I may fairly refer the reader to the paragraphs alluded to above as containing sufficient evidence of their correctness, and I will here only add a single argument, which seems to leave the question where I have placed it.

It seems to me that there is much confusion of ideas in the general statement of the variability of species so often repeated lately. If species do not exist at all, as the supporters of the transmutation theory maintain, how can they vary? and if individuals alone exist, how can the differences which may be observed among them prove the variability of species? The fact seems to me to be that while species are based upon definite relations among individuals which differ in various ways among themselves,

each individual, as a distinct being, has a definite course to run from the time of its first formation to the end of its existence, during which it never loses its identity nor changes its individuality, nor its relations to other individuals belonging to the same species, but preserves all the categories of relationship which constitute specific or generic or family affinity, or any other kind or degree of affinity. *To prove that species vary it should be proved that individuals born from common ancestors change the different categories of relationship which they bore primitively to one another.* While all that has thus far been shown is, that there exists a considerable difference among individuals of one and the same species. This may be new to those who have looked upon every individual picked up at random, as affording the means of describing satisfactorily any species; but no naturalist who has studied carefully any of the species now best known, can have failed to perceive that it requires extensive series of specimens accurately to describe a species, and that the more complete such series are, the more precise appear the limits which separate species. Surely the aim of science cannot be to furnish amateur zoölogists or collectors, a *recipe* for a ready identification of any chance specimen that may fall into their hands. And the difficulties with which we may meet in attempting to characterize species do not afford the first indication that species do not exist at all, as long as most of them can be distinguished, as such, almost at first sight. I foresee that some convert to the transmutation creed will at once object that the facility with which species may be distinguished is no evidence that they were not derived from other species. It may be so. But as long as no fact is adduced to show that any one well known species among the many thousands that are buried in the whole series of fossiliferous rocks, is actually the parent of any one of the species now living, such arguments can have no weight; and thus far the supporters of the transmutation theory have failed to produce any such facts. Instead of facts we are treated with marvelous bear, cuckoo, and other stories. Credat Judaeus Apella!

Had Mr. Darwin or his followers furnished a single fact to show that individuals change, in the course of time, in such a manner as to produce at last species different from those known before, the state of the case might be different. But it stands recorded now as before, that the animals known to the ancients are still in existence, exhibiting to this day the characters they exhibited of old. The geological record, even with all its imperfections, exaggerated to distortion, tells now, what it has told from the beginning, that the supposed intermediate forms between the species of different geological periods are imaginary beings, called up merely in support of a fanciful theory. The origin of all the diversity among living beings remains a mystery as totally unexplained as if the book of Mr. Darwin had never been written, for no theory unsupported by fact, however plausible it may appear, can be admitted in science.

It seems generally admitted that the work of Darwin is particularly remarkable for the fairness with which he presents the facts adverse to his views. It may be so; but I confess that it has made a very different impression upon me. I have been more forcibly struck by his inability to perceive when the facts are fatal to his argument, than by anything else in the whole work. His chapter on the Geological Record, in par-

ticular, appears to me, from beginning to end, as a series of illogical deductions and misrepresentations of the modern results of Geology and Palæontology. I do not intend to argue here, one by one, the questions he has discussed. Such arguments end too often in special pleading, and any one familiar with the subject may readily perceive where the truth lies by confronting his assertions with the geological record itself. But since the question at issue is chiefly to be settled by palæontological evidence, and I have devoted the greater part of my life to the special study of the fossils, I wish to record my protest against his mode of treating this part of the subject. Not only does Darwin never perceive when the facts are fatal to his views, but when he has succeeded by an ingenious circumlocution in overleaping the facts, he would have us believe that he has lessened their importance or changed their meaning. He would thus have us believe that there have been periods during which all that had taken place during other periods was destroyed, and this solely to explain the absence of intermediate forms between the fossils found in successive deposits, for the origin of which he looks to those missing links; whilst every recent progress in Geology shows more and more fully how gradual and successive all the deposits have been which form the crust of our earth.—He would have us believe that entire faunæ have disappeared before those were preserved, the remains of which are found in the lowest fossiliferous strata; when we find everywhere non-fossiliferous strata below those that contain the oldest fossils now known. It is true, he explains their absence by the supposition that they were too delicate to be preserved; but any animals from which Crinoids, Brachiopods, Cephalopods, and Trilobites could arise, must have been sufficiently similar to them to have left, at least, traces of their presence in the lowest non-fossiliferous rocks, had they ever existed at all.—He would have us believe that the oldest organisms that existed were simple cells, or something like the lowest living beings now in existence; when such highly organized animals as Trilobites and Orthoceratites are among the oldest known.—He would have us believe that these lowest first-born became extinct in consequence of the gradual advantage some of their more favored descendants gained over the majority of their predecessors; when there exist now, and have existed at all periods in past history, as large a proportion of more simply organized beings, as of more favored types, and when such types as Lingula were among the lowest Silurian fossils, and are alive at the present day.—He would have us believe that each new species originated in consequence of some slight change in those that preceded; when every geological formation teems with types that did not exist before.—He would have us believe that animals and plants became gradually more and more numerous; when most species appear in myriads of individuals, in the first bed in which they are found. He would have us believe that animals disappear gradually; when they are as common in the uppermost bed in which they occur as in the lowest, or any intermediate bed. Species appear suddenly and disappear suddenly in successive strata. That is the fact proclaimed by Palæontology; they neither increase successively in number, nor do they gradually dwindle down; none of the fossil remains thus far observed show signs of a gradual improvement or of a slow decay.—He would have us believe that

geological deposits took place during the periods of subsidence; when it can be proved that the whole continent of North America is formed of beds which were deposited during a series of successive upheavals. I quote North America in preference to any other part of the world, because the evidence is so complete here that it can only be overlooked by those who may mistake subsidence for the general shrinkage of the earth's surface in consequence of the cooling of its mass. In this part of the globe, fossils are as common along the successive shores of the rising deposits of the Silurian system, as anywhere along our beaches; and and each of these successive shores extends from the Atlantic States to the foot of the Rocky Mountains. The evidence goes even further; each of these successive sets of beds of the Silurian system contains peculiar fossils, neither found in the beds above nor in the beds below, and between them there are no intermediate forms. And yet Darwin affirms that "the littoral and sub-littoral deposits are continually worn away, as soon as they are brought up by the slow and gradual rising of the land within the grinding action of the coast waves." Origin of Species, p. 290.—He would also have us believe that the most perfect organs of the body of animals are the product of gradual improvement, when eyes as perfect as those of the Trilobites are preserved with the remains of these oldest animals.—He would have us believe that it required millions of years to effect any one of these changes; when far more extraordinary transformations are daily going on, under our eyes, in the shortest periods of time, during the growth of animals.—He would have us believe that animals acquire their instincts gradually; when even those that never see their parents, perform at birth the same acts, in the same way, as their progenitors.—He would have us believe that the geographical distribution of animals is the result of accidental transfers; when most species are so narrowly confined within the limits of their natural range, that even slight changes in their external relations may cause their death. And all these, and many other calls upon our credulity, are coolly made in the face of an amount of precise information, readily accessible, which would overwhelm any one who does not place his opinions above the records of an age eminently characterized for its industry, and during which, that information was laboriously accumulated by crowds of faithful laborers.

It would be superfluous to discuss in detail the arguments by which Mr. Darwin attempts to explain the diversity among animals. Suffice it to say, that he has lost sight of the most striking of the features, and the one which pervades the whole, namely, that there runs throughout Nature unmistakable evidence of thought, corresponding to the mental operations of our own mind, and therefore intelligible to us as thinking beings, and unaccountable on any other basis than that they owe their existence to the working of intelligence; and no theory that overlooks this element can be true to nature.

There are naturalists who seem to look upon the idea of creation, that is, a manifestation of an intellectual power by material means, as a kind of bigotry; forgetting, no doubt, that whenever they carry out a thought of their own, they do something akin to creating, unless they look upon their own elucubrations as something in which their individuality is not concerned, but arising without an intervention of their mind, in conse-

quence of the working of some "bundles of forces," about which they know nothing themselves. And yet such men are ready to admit that matter is omnipotent, and consider a disbelief in the omnipotence of matter as tantamout to imbecility; for, what is the assumed power of matter to produce all finite beings, but omnipotence? And what is the outcry raised against those who cannot admit it, but an insinuation that they are *non-compos?* The book of Mr. Darwin is free of all such uncharitable sentiments towards his fellow-laborers in the field of science; nevertheless his mistake lies in a similar assumption that the most complicated system of combined thoughts can be the result of accidental causes; for he ought to know, as every physicist will concede, that all the influences to which he would ascribe the origin of species are accidental in their very nature, and he must know, as every naturalist familiar with the modern progress of science does know, that the organized beings which live now, and have lived in former geological periods, constitute an organic whole, intelligibly and methodically combined in all its parts. As a zoölogist he must know in particular, that the animal kingdom is built upon four different plans of structure, and that the reproduction and growth of animals takes place according to four different modes of development, and that unless it is shown that these four plans of structure, and these four modes of development, are transmutable one into the other, no transmutation theory can account for the origin of species. The fallacy of Mr. Darwin's theory of the origin of species by means of natural selection, may be traced in the first few pages of his book, where he overlooks the difference between the voluntary and deliberate acts of selection applied methodically by man to the breeding of domesticated animals and the growing of cultivated plants, and the chance influences which may effect animals and plants in the state of nature. To call these influences "natural selection," is a misnomer which will not alter the conditions under which they may produce the desired results. Selection implies design; the powers to which Darwin refers the order of species, can design nothing. Selection is no doubt the essential principle on which the raising of breeds is founded, and the subject of breeds is presented in its true light by Mr. Darwin; but this process of raising breeds by the selection of favorable subjects, is in no way similar to that which regulates specific differences. Nothing is more remote from the truth than the attempted parallelism between the breeds of domesticated animals and the species of wild ones. Did there exist such a parallelism, as Darwin maintains, the difference among the domesticated breeds should be akin to the differences among wild species, and afford a clue to determine their relative degree of affinity by a comparison with the pedigrees of well-known domesticated races. Again, if there were any such parallelism, the distinctive characteristics of different breeds should be akin to the differences which exist between fossil species of earlier periods and those of the same genera now living. Now let any one familiar with the fossil species of the genera Bos and Canis, compare them with the races of our cattle and of our dogs, and he will find no correspondence whatever between them; for the simple reason that they do not owe their existence to the same causes. It must therefore be distinctly stated that Mr. Darwin has failed to establish a connection between the mode of raising domesticated

breeds, and the cause or causes to which wild animals owe their specific differences.

It is true, Mr. Darwin states that the close affinity existing among animals can only be explained by a community of descent, and he goes so far as to represent these affinities as evidence of such a genealogical relationship; but I apprehend that the meaning of the words he uses has misled him into the belief that he had found the clue to phenomena which he does not even seem correctly to understand. There is nothing parallel between the relations of animals belonging to the same genus or the same family, and the relations between the progeny of common ancestors. In the one case we have the result of a physiological law regulating reproduction, and in other affinities which no observation has thus far shown to be in any way connected with reproduction. The most closely allied species of the same genus or the different species of closely allied genera, or the different genera of one and the same natural family, embrace representatives which at some period or other of their growth resemble one another more closely than the nearest blood relations; and yet we know that they are only stages of development of different species distinct from one another at every period of their life. The embryo of our common fresh water turtle, *Chrysemis picta*, and the embryo of our snapping turtle, *Chelydra serpentina*, resemble one another far more than the different species of Chrysemis in their adult state, and yet not a single fact can be adduced to show that any one egg of an animal has ever produced an individual of any species but its own. A young snake resembles a young turtle or a young bird much more than any two species of snakes resemble one another; and yet they go on reproducing their kinds, and nothing but their kinds. So that no degree of affinity, however close, can, in the present state of our science, be urged as exhibiting any evidence of community of descent, while the power that imparted all their peculiarities to the primitive eggs of all the species now living side by side, could also impart similar peculiarities with similar relations, and all degrees of relationship, to any number of other species that have existed. Until, therefore it can be shown that any one species has the ability to delegate such specified peculiarities and relations to any other species or set of species, it is not logical to assume that such a power is inherent in any animal, or that it constitutes part of its nature.* We must look to the original power that imparted life to the first being for the origin of all other beings, however mysterious and inaccessible

* The difficulty of ascertaining the natural limits of some species, and the mistakes made by naturalists when describing individual peculiarities as specific, has nothing to do with the question of the origin of species, and yet Darwin places great weight, in support of his theory, upon the differences which exist among naturalists in their views of species. Some of the metals are difficult to distinguish, and have frequently been mistaken, and the specific differences of some may be questioned; but what could that have to do with the question of the origin of metals, in the minds of those who may doubt the original difference of metals? Nothing more than the blunders of some naturalists in identifying species with the origin of species of animals and plants. The great mischief in our science now lies in the self-complacent confidence with which certain zoölogists look upon a few insignificant lines, called diagnoses, which they have the presumption to offer as characteristics of species, or, what is still worse, as checks upon others to secure to themselves a nominal priority. Such a treatment of scientific subjects is unworthy of our age.

the modes by which all this diversity has been produced may remain for us. The production of a plausible explanation is no explanation at all, if it does not cover the whole ground.

All attempts to explain the origin of species may be brought under two categories: viz. 1st, some naturalists admitting that all organized beings are created, that is to say, endowed from the beginning of their existence with all their characteristics, while 2d, others assume that they arise spontaneously. This classification of the different theories of the origin of species, may appear objectionable to the supporters of the transmutation theory; but I can perceive no essential difference between their views and the old idea that animals may have arisen spontaneously. They differ only in the modes by which the spontaneous appearance is assumed to be effected; some believe that physical agents may so influence organized beings as to modify them—this is the view of DeMaillet and the Vestiges of Creation; others believe that the organized beings themselves change in consequence of their own acts, by changing their mode of life, etc., this is the view of Lamarck; others still assume that animals and plants tend necessarily to improve, in consequence of the struggle for life, in which the favored races are supposed to survive; this is the view lately propounded by Darwin. I believe these theories will, in the end, all share the fate of the theory of spontaneous generations so called, as the facts of nature shall be confronted more closely with the theoretical assumptions. The theories of DeMaillet, Oken, and Lamarck are already abandoned by those who have adopted the transmutation theory of Darwin; and unless Darwin and his followers succeed in showing that the struggle for life tends to something beyond favoring the existence of certain individuals over that of other individuals, they will soon find that they are following a shadow. The assertion of Darwin, which has crept into the title of his work, is, that favored *races* are preserved, while all his facts go only to substantiate the assertion, that favored *individuals* have a better chance in the struggle for life than others. But who has ever overlooked the fact that myriads of individuals of every species constantly die before coming to maturity? What ought to be shown, if the transmutation theory is to stand, is that these favored individuals diverge from their specific type, and neither Darwin nor any body else has furnished a single fact to show that they go on diverging. The criterion of a true theory consists in the facility with which it accounts for facts accumulated in the course of long-continued investigations and for which the existing theories afforded no explanation. It can certainly not be said that Darwin's theory will stand by that test. It would be easy to invent other theories that might account for the diversity of species quite as well, if not better than Darwin's preservation of favored races. The difficulty would only be to prove that they agree with the facts of Nature. It might be assumed, for instance, that any one primary being contained the possibilities of all those that have followed, in the same manner as the egg of any animal possesses all the elements of the full-grown individual; but this would only remove the difficulty one step further back. It would tell us nothing about the nature of the operation by which the change is introduced. Since the knowledge we now have, that similar

metamorphoses go on in the eggs of all living beings has not yet put us on the track of the forces by which the changes they undergo are brought about, it is not likely that by mere guesses we shall arrive at any satisfactory explanation of the very origin of these beings themselves.

Whatever views are correct concerning the origin of species, one thing is certain, that as long as they exist they continue to produce generation after generation, individuals which differ from one another only in such peculiarities as relate to their individuality. The great defect in Darwin's treatment of the subject of species lies in the total absence of any statement respecting the features that constitute individuality. Surely, if individuals may vary within the limits assumed by Darwin, he was bound first to show that individuality does not consist of a sum of hereditary characteristics, combined with variable elements, not necessarily transmitted in their integrity, but only of variable elements. That the latter is not the case, stands recorded in every accurate monograph of all the types of the animal kingdom upon which minute embryological investigations have been made. It is known, that every individual egg undergoes a series of definite changes before it reaches its mature condition; that every germ formed in the egg passes through a series of metamorphoses before it assumes the structural features of the adult; that in this development the differences of sex may very early become distinct; and that all this is accomplished in a comparatively very short time, extremely short, indeed, in comparison to the immeasurable periods required by Darwin's theory to produce any change among species; and yet all this takes place without any deviation from the original type of the species, though under circumstances which would seem most unfavorable to the maintenance of the type. Whatever minor differences may exist between the products of this succession of generations are all *individual peculiarities*, in no way connected with the essential features of the species, and therefore as transient as the individuals; while the specific characters are forever fixed. A single example will prove this. All the robins of North America now living have been for a short time in existence; not one of them was alive a century ago, when Linnæus for the first time made known that species under the name of Turdus migratorius, and not one of the specimens observed by Linnæus and his cotemporaries was alive when the Pilgrims of the Mayflower first set foot upon the Rock of Plymouth. Where was the species at these different periods, and where is it now? Certainly nowhere but in the individuals alive for the time being; but not in any single one of them, for that one must be either a male or a female, and not the species; not in a pair of them, for the species exhibits its peculiarities in its mode of breeding, in its nest, in its eggs, in its young, as much as in the appearance of the adult; not in all the individuals of any particular district, for the geographical distribution of a species over its whole area, forms also part of its specific characters.* A species is only known when its whole history

* We are so much accustomed to see animals reproducing themselves, generation after generation, that the fact no longer attracts our attention, and the mystery involved in it no longer excites our admiration. But there is certainly no more marvellous law in all nature than that which regulates this regular succession. And upon this law the maintenance of species depends; for observation teaches us that all that is not individual peculiarity is unceasingly and integrally reproduced while all that constitutes individuality, as such, constantly disappears.

has been ascertained, and that history is recorded in the life of individuals through successive generations. The same kind of argument might be adduced from every existing species, and with still greater force by a reference to those species already known to the ancients.

Let it not be objected that the individuals of successive generations have presented marked differences among themselves; for these differences, with all the monstrosities that may have occurred, during these countless generations, have passed away with the individuals, as individual peculiarities, and the specific characteristics alone have been preserved, together with all that distinguishes the genus, the family, the order, the class, and the branch to which the individual belonged. Moreover all this has been maintained through a succession of repeated changes, amounting in each individual to the whole range of transformations, through which an individual passes, from the time it is individualized as an egg, to the time it is itself capable of reproducing its kind, and, perhaps, with all the intervening phases of an unequal production of males and females, of sterile individuals, of dwarfs, of giants, etc., etc., during which there were millions of chances for a deviation from the type. Does this not prove that while individuals are perishable, they transmit, generation after generation, all that is specific or generic, or, in one word, *typical* in them, to the exclusion of every *individual peculiarity* which passes away with them, and that, therefore, while individuals alone have a material existence, species, genera, families, orders, classes, and branches of the animal kingdom exist only as categories of thought in the Supreme Intelligence, but as such have as truly an independent existence and are as unvarying as thought itself after it has once been expressed.

Returning, after this digression, to the question of individuality among Acalephs, we meet here phenomena far more complicated than among higher animals. Individuality, as far as it depends upon material isolation, is complete and absolute in all the higher animals, and there maintained by genetic transmission, generation after generation. Individuality, in that sense, exists only in comparatively few of the Radiates. Among Acalephs it is ascertained only for the Ctenophoræ and some Discophoræ. In others, the individuals born from eggs end by dividing into a number of distinct individuals. In others still, the successive individuals derived from a primary one, remain connected to form compound communities. We must therefore, distinguish different kinds and different degrees of individuality, and may call *hereditary individuality* that kind of independent existence manifested in the successive evolutions of a single egg, producing a single individual, as is observed in all the higher animals. We may call *derivative* or *consecutive individuality* that kind of independence resulting from an individualization of parts of the product of a single egg. We have derivative individuals among the Nudibranchiate Mollusks, whose eggs produce singly, by a process of complete segmentation, several independent individuals. We observe a similar phenomenon among those Acalephs the young of which (Scyphostoma) ends in producing, by transverse division (Strobila), a number of independent free Medusæ (Ephyræ). We have it also among the Hydroids which produce free Medusæ. Next, we must distinguish *secondary individuality*, which is inherent to those individuals arising as

buds from other individuals, and remaining connected with them. This condition prevails in all the immovable Polyparia and Hydraria, and I say intentionally in the immovable ones; for, in the movable communities, such as Renilla, Pennatula, etc., among Polyps, and all the Siphonophoræ among Acalephs, we must still further distinguish another kind of individuality, which I know not how to call properly, unless the name of *complex individuality* may be applied to it. In complex individuality a new element is introduced, that is not noticeable in the former case. The individuals of the community are not only connected together, but, under given circumstances, they act together as if they were one individual, while at the same time each individual may perform acts of its own.

As to the specific differences observed among Acalephs, there is as great a diversity between them as between their individuals. In some types of this class the species are very uniform; all the individuals belonging to one and the same species resembling one another very closely, and exhibiting hardly any difference among themselves, except such as arises from age. This identity of the individuals of one and the same species is particularly striking among the Ctenophoræ. In this order there are not even sexual differences among the individuals, as they are all hermaphrodites. In the Discophoræ proper a somewhat greater diversity prevails. In the first place we notice male and female individuals, and the difference between the sexes is quite striking in some genera, as, for instance, in Aurelia. Next there occur frequent deviations, among them, in the normal number of their parts; their body consisting frequently of one or two spheromeres more than usual, sometimes, even, of double normal number, or of a few less. And yet, year after year, the same Discophoræ reappear upon our shores, with the same range of differences among their individuals. Among Hydroids polymorphism prevails to a greater or less extent, besides the differences arising from sex. Few species have only one kind of individuals. Mostly the cycle of individual differences embraces two distinct types of individuals, one recalling the peculiarities of common Hydræ, the other those of Medusæ; but even the Hydra type of one and the same species may exhibit more or less diversity, there being frequently two kinds of Hydræ united in one and the same community, and sometimes even a larger number of heterogeneous Hydræ. And this is equally true, though to a less extent, of the Medusa type. Yet among Siphonophoræ there are generally at least two kinds of Medusæ in one and the same community. But notwithstanding this polymorphism among the individuals of one and the same community, genetically connected together, each successive generation reproduces the same kinds of heterogeneous individuals, and nothing but individuals linked together in the same way. Surely we have here a much greater diversity of individuals, born one from the other, than is exhibited by the most diversified breeds of our domesticated animals; and yet all these heterogeneous individuals remain true to their species, in once case as in the other, and do not afford the slightest evidence of a transmutation of species.

Would the supporters of the fanciful theories lately propounded, only extend their studies a little beyond the range of domesticated animals, would they investigate the alternate generations of the Acalephs,

the extraordinary modes of development of the Helminth, the reproduction of the Salpæ, etc., etc., they would soon learn that there are, in the world, far more astonishing phenomena, strictly circumscribed between the natural limits of unvarying species, than the slight differences produced by the intervention of men, among domesticated animals, and, perhaps, cease to be so confident as they seem to be, that these differences are trustworthy indications of the variability of species. For my own part I must emphatically declare that I do not know a single fact tending to show that species do vary in any way, while it is true that the individuals of one and the same species are more or less polymorphous. The circumstance, that naturalists may find it dificult to trace the natural limits of any one particular species, or the mistakes they may make in their attempts to distinguish them, has nothing whatsoever to do with the question of their origin.

There is another feature of the species of Acalephs which deserves particularly to be noticed. All these animals are periodical in their appearance, and last for a short period, in their perfect state of development. In our latitude most Medusæ make their appearance, as Ephyræ, early in the Spring, and rapidly enlarge to their full size. In September and October they lay their eggs and disappear; the young hatched from the eggs move about, as Planulæ, for a short time, and then become attached, as Scyphostomes, and pass the winter in undergoing their Strobila metamorphosis. The Ctenophoræ appear also very early, and lay their eggs in the autumn, passing the winter as young, and growing to their full size towards the beginning of the summer. Among the Hydroids there is more diversity in their periodicity. Hydraria are found all the year round; but the Medusæ buds, the free Medusæ, and the Medusaria make their appearance in different seasons, in different species. Some bring forth Medusæ buds and free Medusæ or Medusaria during winter; others, and in our latitude this is the case with by far the largest number of the Hydroids, produce their Medusæ brood in the spring; a few breed later, in the summer or in the autumn; so that, notwithstanding the regularity of their periodical return, Acalephs may be studied, in some condition or other, during the whole year.

When considering Individuality and Specific Differences, as manifested in the class of Acalephs, I have taken an opportunity of showing, upon general grounds how futile the arguments are upon which the theory of transmutation of species is founded. Having now shown that that class is circumscribed within definite limits, I may be permitted to add here a few more objections to that theory, based chiefly upon special grounds, connected with the characteristics of classes. If there is any thing striking in the features which distinguish classes, it is the definiteness of their structural peculiarities; and this definiteness goes on increasing, with new and additional qualifications, as we pass from the class characters to those which mark the orders, the families, the genera, and the species. Granting, for the sake of argument, that organized beings living at a later period may have originated by a gradual change of those of earlier periods, one of the most characteristic features of all organized beings remains totally unexplained by the various

theories brought forward to explain that change; the definiteness of their respective groups, be they ever so comprehensive, or ever so limited, combined with the greatest inequality in their numeric relations. There exist a few thousand Mammalia and Reptiles, and at least three times their number of Birds and Fishes. There may be twenty thousand Mollusks; but there are over a hundred thousand Insects, and only a few thousand Radiates. And yet the limits of the class of Insects are as well defined as those of any other class, with the only exception of the class of Birds which is unquestionably the most definite in its natural boundaries. Now the supporters of the transmutation theory may shape their views in whatever way they please to suit the requirements of the theory, instead of building the theory upon the facts of Nature, they never can make it appear that the definiteness of the characters of the class of Birds is the result of a common descent of all Birds, for the first Bird must have been brother or cousin to some other animal that was not a Bird, since there are other animals besides Birds in this world, to no one of which any bird bears as close a relation as it bears to its own class. The same argument applies to every other class; and as to the facts, they are fatal to such an assumption, for Geology teaches us that among the oldest inhabitants of our globe known, there are representatives of nine distinct classes of animals, which by no possibility can be descendants of one another, since they are cotemporaries.

The same line of argument and the same class of facts forbid the assumption that either the representatives of one and the same order, or those of one of the same family, or those of one of the same genus should be considered as lineal descendants of a common stock; for orders, families and genera are based upon different categories of characters, and not upon more or less extensive characters of the same kind, as I have shown years ago (Vol. I, p. 150 to 163), and numbers of different kinds of representatives of these various groups, make their appearance simultaneously in all the successive geological periods. There appear together Corals and Echinoderms of different families and of different genera in each successive geological formation, and this is equally true for Bryozoa, Brachiopods and Lamellibranchiata, for Trilobites and the other Crustacea, in fact for the representatives of all the classes of the animal kingdom, making due allowance for the period of the first appearance of each; and at all times and in all classes the representatives of these different kinds of groups are found to present the same definiteness in their characteristics and limitation. Were the transmutation theory true, the geological record should exhibit an uninterrupted succession of types blending gradually into one another. The fact is that throughout all geological times each period is characterized by definite specific types, belonging to definite genera, and these to definite families, referable to definite orders, constituting definite classes and definite branches, built upon definite plans. Until the facts of Nature are shown to have been mistaken by those who have collected them, and that they have a different meaning from that now generally assigned to them, I shall therefore consider the transmutation theory as a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency.

Cambridge, June 30, 1860.

2. *Anleitung zur Organischen und Gasanalyse von* J. SCHIEL. Erlangen, 1860. 260 pp., 8vo.—This introduction to elementary organic and gas analysis, by our adopted countryman Dr. Schiel of St. Louis, contains a very full, yet concise, account of all of the approved methods, and is an excellent hand-book of these subjects. Many of the refinements of research which the text-books do not notice, and newly discovered processes or apparatus that are beginning to displace those hitherto in vogue, we find satisfactorily described in these pages. The best and simplest methods for taking the specific gravity of liquids, for determining melting and boiling points are given, and the admirable process of Simpson for the quantitative determination of nitrogen as well as that of Natanson for taking vapor densities are fully described.

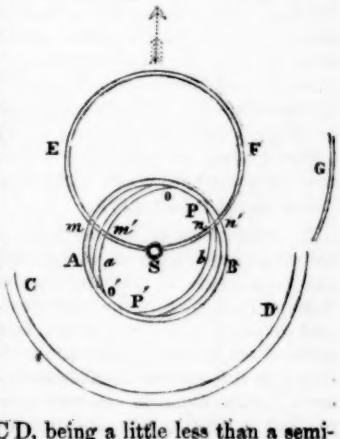
The chapters on Gas Analysis present in a clear manner the important parts of this subject. An instructive section of the book is occupied with an exposition of the mathematical principles which apply to the discussion of experimental results and the correction of errors of observation.

We believe that this treatise will be of great service in the hands of students, and by its careful selection and full description of the methods that unite simplicity and accuracy, will help to inspire them with the *animus* of scientific research.

VI. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Parhelia seen at Weld, Franklin Co., Me.;* by STILLMAN MASTERMAN, (in a letter to the Editors).—*Messrs. Editors:*—I send you the following account of a *parhelion* seen by myself, at this place on the 21st instant, at $10^h\ 45^m$ A. M.

A light haze was spread over the sky at the time, for the most part so thin as to be scarcely visible, but at a few points coming out in lenticular patches of well-marked *cirrus.* When first noticed at the time above stated, the meteor presented the appearance delineated in the accompanying drawing. The circle A B was about 45° in diameter, having the sun in its centre; and was very brilliant, having the colors of the primary rainbow, but with the red next to the sun. It was surrounded by a bright corona of white, three or four degrees in width: the width of the colored ring A B being equal to the apparent diameter of the solar orb. Two elliptical arcs *a* and *b* were included in A B, having their middles 2° distant from the inner edge of the latter and their extremities falling within it at *o o'* and P P'. In color and width *a* and *b* were precisely like A B and of a like brightness. Concentric with A B was the arc C D, being a little less than a semicircle, on a diameter of 95°, colored like A B with its red next to the



sun, but of twice the width of the latter, and of dazzling brilliancy. The circle E F, with a diameter of about 60°, had its circumference in the sun and its centre vertically above him, [the arrow denoting the position of the vertical passing through the sun,] and consisted of a ring of pure white, quite brilliant, and having a width equal to two-thirds the apparent diameter of the sun. The arc G was concentric with E F, and bore the same proportion in radius to the latter that C D did to A B. It was white like E F, but not nearly so bright. The *parhelia* at *m m'* and *n n'* were equal to the united brilliancy of the rings intersecting at those points.

At $11^h\ 10^m$, all the rings had vanished but A B, which had contracted in diameter and width, and a small portion of C D immediately below the sun.

At $11^h\ 30^m$, A B had regained its former dimensions, with increased brilliancy. C D had entirely vanished.

At $12^h\ 0^m$, A B had contracted to 30° diameter, and become faint. All traces of the meteor had entirely vanished at $1^h\ 30^m$ P. M. The *cirrus* had in the mean time become gradually converted into *cirro-cumulus*, and the thin haze had completely vanished.

Weld, Franklin Co., Maine, May 28th, 1860.

2. *Tolles' improved Microscope Objectives.*—It has been generally supposed that the further improvement of object-glasses for the microscope was to be sought in the production of flint glass of high dispersive power. I am happy to inform your readers that Mr. R. B. Tolles of Canastota, N. Y., has boldly discarded the use of flint glass of unusual dispersive power and has discovered a method of constructing glasses of as large aperture as have ever yet been made by any artist, using only such glass of uniform density as is made for ordinary optical purposes.

I have recently received from Mr. Tolles a *one-third inch objective*, constructed on his plan, which has an aperture of 100° and is remarkable for the great amount of light transmitted even at the extreme borders of the lens. This objective is well corrected for chromatic and spherical aberration and gives excellent definition of blood corpuscles as well as of other tests appropriate for such an objective. This is a remarkably fine glass for ordinary investigations in minute anatomy and pathology. Mr. Tolles makes a one-fourth inch objective of 140° to 160° aperture, and a one-eighth inch of 160° to 175° aperture. I am inclined to think Mr. Tolles' glasses are unsurpassed by any object-glasses for the microscope that have yet been made in this country or elsewhere. M. C. White.

New Haven, June 10, 1860.

3. *Application of Photography in construction of Micrometers.*—The successful application of photography in the construction of micrometers, has been made by Mr. Clarence Morfit of the U. S. Assay Office, New York. It is merely the reduction of a large scale of exact dimensions and divisions to a definite size suitable for microscopic instruments. A scale of ten inches divided into inches and tenths of an inch has been reduced in this manner to one-twentieth of an inch, thus making its smallest divisions equal to one two-thousandth part of an inch square. The method is simple, accurate and economical. Moreover, the micrometer has the advantage of giving the exact measurement of the object in frac-

tions of an inch and at the same time determines the power of the microscope itself.

4. *Geological Survey of California.*—The Act of the California legislature (approved April 24, 1860) appoints Prof. J. D. Whitney State Geologist with a salary of $6000 per annum. He appoints his own assistants with the approval of the Governor. Twenty thousand dollars are appropriated to the payment of expenses incurred in the survey. The provisions of the Act respecting the annual and final Reports are eminently judicious, especially that part which directs the Governor and Secretary of State to cause the volumes of the geological survey to be sold for the benefit of the Common School fund of the State.

No state geological survey was ever more auspiciously inaugurated, wisely provided for, or fraught with more interesting scientific and practical problems. It is understood that Prof. Whitney will commence his Californian labors in the coming autumn.

5. *Total Solar Eclipse of July* 18, 1860.—The American Nautical Almanac Commission has sent three observers to the Cumberland House, British America, to take note of this eclipse. This is a station of the Hudson's Bay Company on the West side of Pine Island Lake, in lat. 54° N., long. 102° 40′ W.

Another party of observers, consisting of Prof. Stephen Alexander, of Princeton, N. J., President F. A. P. Barnard, of Oxford, Mississippi, Prof. C. S. Venable, of North Carolina, Prof. A. W. Smith of the Naval Academy, Annapolis, Md., and Lieut. E. D. Ashe, R. N., of the Quebec Observatory, embarked June 28th at New York in the U. S. steamer Bibb, Lt. Commanding Alex. Murray, to proceed to Cape Chadleigh, Labrador.

6. *Newport Meeting of the American Association for the Advancement of Science.*—The next meeting of the Association will be held at Newport, R. I., commencing on Wednesday the first of August. The officers of the meeting are: President, Isaac Lea, Esq. of Philadelphia; Vice President, Dr. B. A. Gould of Cambridge; General Secretary, Prof. Joseph LeConte of Columbia, S. Ca.; Treasurer, Dr. A. L. Elwyn of Philadelphia.

It will be remembered that Prof. Henry, at the request of the Association, will deliver a discourse commemorative of the life and scientific labors of Dr. Robert Hare. Prof. Bache will also by appointment give an address on the Gulf Stream; while Prof. Leidy was requested to prepare a discourse on the extinct Reptilia and Mammalia of North America.

There are many reasons why this may be looked forward to as one of the most attractive and promising meetings of the Association ever held.

7. *Letter from John McCrady, Esq., Charleston, on the* Lingula pyramidata *described by Mr. W. Stimpson*, vol. xxix, p. 444.—Dear Stimpson: I believe you are connected with the Zoological department of Silliman's Journal during the absence of Prof. Dana. You have also, I see, redescribed there our Lingula, long known to naturalists but not before described. It was found more than ten years ago on our coast by the Rev. Thomas J. Young. A living specimen was also found by Mr. Burkhardt on Sullivan's Island when Prof. Agassiz had a laboratory there. I have never seen a specimen during all the time I have resided on Sullivan's Island. I am very glad you have distinguished and described it, since I suppose the Beaufort species can hardly be different from the South Carolinian.

I have found quite lately something very interesting in connection with this *Lingula pyramidata*. You know that up to this time nothing has been known to science of the development of the Brachiopoda. I have within a few days discovered an embryo unlike anything known, and which has at once so many affinities with the Bryozoa and the Brachiopoda, that I believe it to be an embryo Brachiopod, and very probably the young of *Lingula pyramidata*. Imagine an equilateral, thin, hyaline, straight-hinged bivalve shell, elliptical in outline, and with valves very flat. Through this perfectly transparent shell is visible a lining membrane or rather the borders of such a membrane, which is the mantle. Within this and near the hinge, a large flask-shaped body containing a digestive cavity surrounded by a dark mass. This cavity extends into the neck of the bottle shaped cavity (œsophagus) and terminates towards the gap of the shell in a mouth. From the opposite or basal end of the digestive cavity goes off a pretty long intestine which turns first to the left, makes several convolutions and terminates in an anus on the right side between the two valves of the shell. The mouth lies on a somewhat triangular prolongation of the body-wall which rests with its apex towards the gap of the shell in the dorsal valve. The borders right and left of this homologue of the arms are fringed each about six cirrhi, the hindermost being the longest.

The animal thus constituted, when quiet withdraws its whole body, cirrhi and all, within its bivalve shell, which is tightly closed; but when in motion the shell is distinctly opened and the gap of the valves is plainly visible even to the naked eye. Through their aperture are thrust out the cirrhi, about twelve in number, which then arrange themselves in circular funnel-like manner precisely as in a Bryozoan polyp, and by a motion plainly ciliary, and with its cirrhi or tentacula thus extended the embryo swims through the water with considerable rapidity.

The cirrhi of this embryo I take to be the homologues of the cirrhi of Brachiopoda, and of Cristatella, and of the tentacula of Bryozoa generally. The rest of the structure, especially the anus on the right and the shape of the shell, point, I think, to Lingula embryo with cirrhi extended about a line in length. There is no trace of a peduncle. It appears to me that this must set at rest all difficulty about the approximation of Bryozoa and Brachiopoda, as proposed by Agassiz and others. A somewhat fuller description of this young animal, with a figure, will be published in the forthcoming issues of the Elliott Society of Charleston.

Charleston, S. C., June 18th, 1860.

8. *The Fusion and Casting of Platinum* on a considerable scale has been accomplished by Messrs. Deville and Debray. At the sitting of the *Académie des Sciences* at Paris, June 4, they exhibited (1.) Two ingots of platinum weighing together 25 kilograms, fused in the same fire and cast in an ingot mould of cast iron. The surface of the metal shows evidence of perfect fluidity and carries the impression of characters engraved on the surface of the mould.

(2.) A toothed wheel, of platinum cast in ordinary founders' sand was also shown. This was cast in the mode common for cast iron in a two-part flask with a sprue and vent holes as usual.

The metal used they obtained by the dry mode from the crude platinum and platinum-money which the Russian Government had placed at their disposal; the details of the process being reserved for a second communication to the Academy.—*Comptes Rendus*, June 14, l, 138.

9. *New Arctic Expedition by* Dr. I. I. HAYES.—Dr. Hayes, well known as the companion of Kane and the author of "An Arctic Boat Voyage," leaves Boston in a small vessel with seventeen companions for Smith's Sound, early in July, designing to test the views respecting the open sea which he has already expressed in detail in this Journal in an article entitled "Observations on the practicability of reaching the North Pole."* This new expedition of Dr. Hayes is equipped by private liberality, and goes out under the auspices of the American Geographical Society.

In this connexion we may mention the proposal of Mr. PARKER SNOW of London, well known for his Arctic researches, to set on foot a new exploration to search for further traces of the Franklin Expedition. Mr. Snow has prepared a paper setting forth his plans and motives in detail, a copy of which we have received and placed before the American Geographical Society at a late sitting, by whom it has been officially communicated to Dr. Hayes, just on the eve of his departure.

10. *Constitution and By-Laws of and List of Officers and Members of the Chicago Academy of Sciences.* Chicago, Illinois, 1859.—This flourishing young association of naturalists was organized in 1857, and incorporated March, 1859. Its establishment affords another gratifying proof of the rapid progress of science at the West.

11. *Personal.*—Prof. A. D. BACHE, Superintendent of the U. S. Coast Survey, was recently chosen a Foreign Member of the Royal Society of London. Of the fifty foreign members, three are now Americans, viz., Professors Peirce, Agassiz, and Bache.

Prof. DANA has been in Switzerland since early in May, and will remain among the mountains until his return, toward the close of summer. His health is slowly but steadily improving.

Prof. BENJAMIN PEIRCE, of Cambridge, has sought relief from his too severe labors in a voyage to Europe by packet. His numerous friends will be pained to learn that his health was much impaired.

Prof. J. P. COOKE, of Cambridge, lately read before the Chemical Society of London a paper on the compounds of antimony and zinc ($SbZn^2$ $SbZn^3$), designed to show that crystalline form is not a necessary indication of definite chemical composition. A copy of this paper from the Author reached us at too late a date for publication in the present number of this Journal.

The gold medals of the London Geographical Society have been awarded (May 28), at the suggestion of Sir R. I. Murchison, to LADY FRANKLIN and SIR LEOPOLD MCCLINTOCK;—to the first in consideration of the deeds of her husband and in recognition of her noble-minded devotion; and to the second for his discovery of the fate of the Erebus and Terror and the accompanying additions to Arctic geography.

LADY FRANKLIN is announced as expected soon to arrive in the United States as the guest of Henry Grinnell, Esq., Vice-President of the American Geographical Society.

* Vol. xxvi, No. 78, 305, Nov. 1858.

12. *To our Correspondents* we owe an apology for the unavoidable postponement to a succeeding Number of several valuable papers, crowded out by the unexpected length of the Articles in the present issue. Even the addition of a full sheet has not sufficed to embrace our customary lists of new books, or a considerable mass of scientific intelligence, proceedings of societies, bibliography, &c., already in type.

13. *Obituary.*—The Rev. BADEN POWELL, Professor of Geometry in the University of Oxford and equally distinguished as a mathematician and physicist, died there in June.

CHARLES GOODYEAR, widely known as the discoverer of "*vulcanization*" of Caoutchouc died in New York, July 1st, aged fifty-nine years.

CANADIAN NATURALIST AND GEOLOGIST, Dec., 1859. Vol. IV.—p. 411, Notes on land and sea birds observed around Quebec; *J. M. LeMoine.*—p. 414, Chemical Geology; *Hunt.*—p. 426, Fossils of the Chazy limestone, with new species; *Billings.*—p. 494, A list of the Coleoptera found in the vicinity of Montreal; *D'Urban.*—Feb. 1860, Vol. V.—p. 1, Devonian plants of Canada; *Dawson.*—p. 14, List of plants found near Prescott, C. W.; *Billings.*—p. 24, Tubicolous Marine Worms of the Gulf of St. Lawrence; *Dawson.*—p. 30, Marine Algæ; *Kemp.*—p. 69, Palæozoic fossils; *Billings.* April.—p. 81, Natural History of the Valley of the River Rouge; *D'Urban.*—p. 100, Review of Darwin; *Dawson.*—p. 120, Sketch of the life of Mr. David Douglas.—p. 132, On the Silurian and Devonian rocks of Nova Scotia; *Dawson.*—p. 144, New fossils from the Silurian of Nova Scotia.

PROCEEDINGS PHILADELPHIA ACAD. NAT. SCI., 1860.—p. 81, Notice of the death of Dr. Hallowell.—p. 82, Habits of an Ocelot kept on ship-board; *Wilson.*—p. 85, Corrected numbers of Unionidæ; *Lea.*—On Hyalonema mirabilis from Japan; *Leidy.*—p. 86, Donation of 28,000 specimens of birds, by Dr. T. B. Wilson.—p. 88, On the coloring matter of the Nacre in Uniones; *Lea.*—p. 89, New South American Unionidæ; *Lea.*—p. 92, New Uniones and Melaniæ; *Lea.*—p. 93, New Cretaceous fossils from New Jersey; *Gabb.*—p. 96, Experiments in chemical geology; *Lesley.*—On Trichina spiralis; *Leidy.*—p. 97, On the palpi of S. American Anodons; *Lea.*—On a gneiss boulder in Orange Co., N. Y.; *Lesley.*—p. 98, New botanical locality; *Leidy.*—On the Albertite of New Brunswick; *Rogers.*—Experiments in Binocular Vision; *Rogers.*—p. 100, Conspectus piscium, etc., the Sicydianæ collected on the North Pacific Expedition; *Theo. Gill.*—p. 102, Monograph of the genus Labrosomus, Sw.; *Gill.*—p. 108, Monograph of the genus Labrax, Cuv.; *Gill.* (Mr. Gill finds our American species generically distinct from the European type. Our striped basse he places in the genus *Roccus*, and the white perch in *Morone*—names adopted from Mitchell.)—p. 120, Monograph of the Philypni; *Gill.*—p. 126, Notice of geological discoveries made by Capt. J. H. Simpson; *Meek* and *Engelman.*—p. 132, Catalogue of Birds collected on the Isthmus of Darien by the expedition under Lt. Michler, with notes and descriptions of new species; *Cassin.*

JOURNAL OF THE ACADEMY OF NATURAL SCIENCES OF PHILADELPHIA, New Series, Vol. IV, Pt. III, March, 1860.—Descriptions of Edotic Unionidæ, 13 plates; *I. Lea.*—New Cretaceous and Eocene fossils from Mississippi and Alabama (2 plates); *T. A. Conrad.*—New Cretaceous and Triassic fossils (a plate); *Gabb.*—Reflections upon the nature of the temporary star of the year 1572,—an application of the nebular hypothesis; *Wilcocks.* W. S.

PROCEEDINGS BOSTON SOC. NAT. HIST., 1860.—p. 226, On two Birds from Bogota—*Turdus minimus* (Lafresnaye) and *Vireo Bogotensis* (Bryant); *Dr. Henry Bryant.*—p. 227, Fossiliferous slate and sandstone (Devonian) from the Dennis river, Maine; *Prof. W. B. Rogers.*—p. 228, Spines of Siluroid fishes from a whale's blubber; *Capt. Atwood.*—p. 229, "Cocoa-nut pearl;" *Dr. C. F. Winslow.*—p. 229, Discussion on the subject of Vision; *Dr. J. B. Jeffries, Prof. W. B. Rogers,* and *Dr. Gould*—p. 231, Discussion on the theory of Darwin; *Agassiz, Emerson,* and *Prof. W. B. Rogers.*—p. 235, Description of Hoplocampa rubi, by the late *Dr. T. W. Harris;* with remarks on its history by *Noyes Darling, Esq.; Mr. Scudder.*—p. 236, Geological map of Vermont; *Mr. C. H. Hitchcock,* with remarks by *Prof. W. B. Rogers.*—p. 239, Section of an elephant's tooth as an example of *Osteo-dentine; Dr. White.*—p. 240, Laws of fracture of a thick glass tube; *Prof. W. B. Rogers.*